NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

Progress.

WITH this Number, Natural Science enters on its fourth volume. Two years of experiment, and of those difficulties incidental to all new publications, are now happily ended. Without deviating from the path originally marked out for it, this venture has secured for itself a basis of which the firmness may be measured by its wide extension. For this result, far more fortunate than they anticipated, the promoters are indebted less to their own labours than to the kindly aid so constantly offered them by an ever-swelling stream of contributors, and to the warm welcome extended by an ever-widening circle of readers. To all of these, whether at home in England or far off in her scattered colonies, whether fellow-workers of our own race in America or workers joined with us in the pursuit of a common aim in all foreign lands from Germany to Japan—to one and all this Number will bear our cordial thanks for the past, and will express that gratitude which is a lively sense of favours to come.

To those of our readers and helpers who have not been with us from the beginning, and who have found difficulty in obtaining our earlier numbers, we should like to take this opportunity of restating our aims. We have been from the beginning, and we still hope to be, as our sub-title indicates, a review of Scientific Progress. Month by month we bring to the specialist a critical account of what is being done in other lines than his own; while, for the general reader, the specialist himself is constantly invited to render more accessible his stores of knowledge. As examples of such work, we may refer to the articles in this number on Cell-Division and on the Fucaceæ. We endeavour also to search out items of interest that are hidden away in unlikely volumes or in publications not easily obtained. Chiefly, in this respect, do we attempt to give the news of our own local scientific societies and of similar bodies scattered throughout the British Dominions. And here it is that we need most of that friendly

aid of which we have already spoken. It is obviously impossible for a scientific monthly to emulate the daily newspaper, to retain paid correspondents in every city of the globe, or to send a special commissioner on a roving tour. But with the goodwill of our colleagues NATURAL SCIENCE may easily be made a bond of union, and a means of communication between naturalists of our own race in all parts of the world. Not that we would in any way pass over foreign labourers in the same field; but if we can make our summary of British Scientific Progress fairly complete, then our Journal will be of all the greater use to those who have not had the rare good-fortune to be born Britons.

But it is not our only object to retail the results of investigation, and to summarise the news of the world of Natural Science. We do not merely wish to review scientific progress; but we hope to assist it. And in this hope we have polished our shield bright, and set our lance in rest. May all shams and bugbears, when they see themselves mirrored as they truly are, sink to the nothingness of which they are made; and may the more enduring obstacles to progress be, if not shivered, at least a little shocked by our onset! Of course this will gain us enemies as well as friends; but that is the more evidence of success, and, after all, we hope our enemies will not be very bitter, since we attack principles not persons, measures not men. It is in pursuit of this aim that we open our columns to those whom authority seems to treat too hardly, even if we do not always agree with them ourselves. It is not always the popular view that is the right one, and the history of Science shows too many instances of folly becoming wisdom, for even an editor to feel his judgment infallible.

F.R.S.—F.L.S.—F.G.S.—F.Z.S.

THE Royal Society is again under fire. At the Anniversary Dinner the President himself deplored internal but obvious evidences of its decay, and a correspondent of *The Times* was not slow to take up the tale. His article, which appeared on December 2, emphasises Lord Kelvin's words and adds some exceedingly outspoken criticisms. The Society is falling, he says, more and more into the hands of biologists, while workers in other branches of science no longer lay their papers before it but before those societies specially interested in their subjects. The cause of this lies, he maintains, "in the excessive influence of a small body of permanent officials, in the practical effacement of the non-official council, and in the consequent government of the Society by cliques instead of by the common-sense of the scientific community at large." As the autocrats in question he denounces the Dons of Cambridge (Trinity College) and the Professors of South Kensington. The "undue preference" shown in the disposal of the Government grant next receives his censure. Especially does he blame the Solar Physics Committee for drawing money

from this grant, despite the fact that they have since 1879 received an independent annual subsidy from the Government of no less than £500. It is, however, a little rash of the critic to wish that the distribution of this grant should be taken out of the hands of the Council; for if it were, as he suggests, divided among the various seats of learning about the country, the same difficulty of cliquism would, we fear, occur. No administrative body can wholly escape this charge.

There is another matter that might well have been touched on by *The Times* denunciator. The methods of election pursued, not only by the Royal but by many of our other learned societies, are exceedingly open to criticism. The mystic letters placed at the head of this article have too long imposed on the credulity of the public. It is too commonly supposed that they are symbols of vast erudition on the part of their happy possessors. Yet, with the exception of "F.R.S.," all these titles are obtainable by little more than simple purchase. The fellowship of the Royal Society comes, it is true, partly as the reward of original work, but mainly by the backing of influential friends. A glance at the list of that Society might possibly dispel the illusion that all its fellows were men of great learning. As for those who are really worthy of election, we do not deny that the majority of them get into the Society in course of time; but there are many others whose social position or personal peculiarities are, unjust as it may seem, allowed to tell against them. Indeed the whole system of election into the Royal Society seems to be in need of improvement. It may be legitimate, but it is hardly self-respecting for a man to suggest his own name as a candidate and then to tout round for support. So derogatory a proceeding should be impossible. Why should not the Society, without regard for personal feeling, select the twelve best men of the year, and let the election come as a surprise to the candidate? The distinction would at least be more honourable.

But it is possible that mere honour is not the only inducement to candidates for the fellowship of the Royal Society. Not this alone would cause a man to get his name put up year after year by some obliging friend, until common-sense or pique urged him to withdraw from the unsuccessful attempt. May not a deeper reason be sometimes found in the commercial value of the Fellowship, not only of the Royal but of the other Societies? We all know that F.R.S. means "Fees Raised Since," while not a few professional men could tell us, if they chose, what pecuniary profit accrued to them from the possession of any of the other magic letters. This is not just, either to the Societies or the public. Election does not and should not imply any professional ability on the part of the candidate. The tailor's cutter, when asked for a reference, replied, "Bring me some cloth and a pair of shears, and you'll want no reference." Such should be the attitude of self-respecting professional men; such is

the attitude of the single-hearted devotee of science. Fellowships may have a value in commercial circles; but no number of them can raise the reputation of a master among his colleagues, either in his own country or abroad.

FONA OR FUDDLEITE. A NEW MINERAL.

IN Mrs. French-Sheldon's entertaining volume, "Sultan to Sultan," in which she records her expedition to Kilima-njaro, there are given (p. 324) two analyses, which we have in vain endeavoured to understand. They are the work of the Royal College of Science, where they were made "through the joint courtesy of Professor Judd and Professor T. E. Thorpe, February 22, 1892." The signature of "T. E. Thorpe" is placed beneath each of them. We beg to quote the formula for the former, which is called "salt stone," as a New Year's puzzle to our readers:—

$$2\,Na_2O\,3\,W_2\,4\tfrac{1}{2}\,O.$$

The mineral, on the strength of this formula, is said to be "*simply* a hydrated sesquicarbonate of soda." Simply that! Where has the tungsten come from, and whither has the carbon gone? How does Professor Thorpe square $4\frac{1}{2}$ O with the atomic theory? And what is the meaning of Nag°, which figures in both analyses?

The second analysis is of a substance called "Tobacco stone," and this seems to be a more complex material, as it is called by Professor Thorpe "fona, mixed with the carbonate of lime and magnesia, a trace of common salt and clay." There is 29·92 per cent. of nagging soda and only 30·48 of carbon dioxide; so, apparently, this up-to-date alkali is satisfied with such a remarkably small proportion of acid that even out of this 30 per cent. it can spare some for the magnesia and lime. If this had been an ordinary compound there must have been a fine scramble for the carbon dioxide between the soda, lime, and magnesia.

Professor Thorpe very justly gives a new name to the material which has yielded these quaint analyses, for we cannot find "fona" in any mineralogical index. As the word is twice repeated, it can hardly be a misprint, though it has a certain resemblance to trona. Fuddleite would, perhaps, have been a more appropriate name. Would some chemist kindly let us know whether the analysis appears clear to him, though we hardly like to invite anyone to a task which has given us cause to doubt our own sanity? Perhaps some interpreter of dreams and expounder of oracles will tell us that in place of "Signed, T. E. Thorpe," there ought to be substituted "Signed, T. E. Thorpe, revised and emended by M. French-Sheldon."

THE VIVISECTION PROBLEM: SELF-HELP OR LAISSEZ-FAIRE?

THE Anti-vivisectionists have been bestirring themselves of late. They have had a meeting and made many speeches. They have

petitioned against the establishment of a Pasteur Institute in India. And that go-ahead Ladies' Club, or club of go-ahead ladies, *The Pioneer*, among its numerous subjects for discussion has lately included vivisection, and after an exciting debate declared it to be unjustifiable.

The justifiability of this practice depends on questions that have been questions since the beginning of civilisation, and that are likely to remain without their answer for many centuries to come. We at least do not presume to give an opinion. The above circumstances have, however, suggested one or two reflections that may be of service to both parties.

Everyone that knows anything of the progress of science, in whatever department it may be, is aware that it is effected by circuitous and often retrogressive paths. Many are the blind alleys up which we wander, many the pits into which we fall. What scientific man dares to say that he has never made a mistake? If such there be, we have small regard for him: be sure that he has never made a discovery. It is a cheap jibe to say that Science contradicts herself. That is nothing to be ashamed of. Oliver Wendell Holmes was wiser when he wrote:—

"Don't be consistent, but be simply true!"

What argument is it, then, to say that experiments on living animals have in some cases led to wrong conclusions, or that those who practised them have occasionally conducted unsuccessful operations? Prove that the whole method is unscientific, and that will be a valid argument. But who will be so rash as to maintain this, so long as in all other sciences there are not one but two factors of progress; not only observation, but also experiment?

We should hardly imagine that, at the present day, there could be found anyone who, having a knowledge of the subject, would deny that, however numerous the failures, experiments on living animals have been productive of great benefit to other animals. In both cases Man is naturally included under the term Animal. It is this benefit that is brought forward by the advocates of such experiments in excuse of any pain that may be inflicted by their performance. Benefit is, perhaps, not always derived; on the other hand, pain is by no means always inflicted. Let us assume that the exceptions balance one another. We shall then have left in one scale some amount of pain, some amount of benefit in the other. Now the benefit consists in the alleviation or the removal of pain. But it must be clearly borne in mind that, whereas the pain inflicted is on a finite number of individuals during a certain comparatively short space of time, the pain removed, on the other hand, is on an infinite number of individuals for all the unknown future. Which then is the heavier scale? Surely none will deny that the pain destroyed far outbalances the pain inflicted. Only on the assumption of an immediate millennium, can any other answer be given to the question.

The problem, though still perhaps insoluble, has become a little clearer. Pain and misery are inevitable in this world. For countless ages Nature has worked by woe. But out of anguish there rises advance, and pain is the parent of pleasure. It is thus that the mills of God are grinding, and man cannot stop their mighty wheels. But, may he hasten them? That is the question. A question which involves issues far larger than the justifiability of vivisection, issues such as the right of one individual over another, be the individuals animals or men. A question to which mere expedience has in all ages forced from the vast majority of mankind an affirmative answer. But, a question which, viewed from the peaks of philosophy or the precipices of morality, must still remain without reply.

THE NORTHERN DISTRIBUTION OF *Oxalis cernua*.

IN the recently-issued number of the *Proceedings of the Linnean Society* (Oct., 1893, p. 31), Professor G. Henslow discusses the northern distribution of *Oxalis cernua*, Thunb. This plant is a native of the Cape of Good Hope, but has become distributed, not only on the islands of the Atlantic, as the Bermudas, Canaries, and Madeira, but along both the north and south coasts of the Mediterranean. The first to allude to its occurrence in the northern hemisphere was Father Giacinto, who mentions the plant as being cultivated in the Botanic Garden at Valetta, Malta, in 1806. Professor Viviani records it for North Africa in 1824, naming it *Oxalis libyca*, while A. de Candolle refers (Fl. Calp., 1846) to its introduction into Gibraltar in 1826. About that time it probably arrived in Egypt with the mandarine orange, introduced from Malta by Youssouf Effendy; at the present time it occurs there only in the orange-gardens of Cairo and Esneh. The author suggests that the plant mentioned as cultivated in Malta in 1806—which Zerapha states that his contemporary, Dr. Giacinto, brought from the Cape, for the information of his pupils—was the source of the diffusion of the species in the Mediterranean region. The floral structure of the northern specimens affords an argument in favour of such an origin. While the species is naturally trimorphic (having long-, mid-, and short-styled forms) at the Cape, and also fruits there, it has never been known to bear fruit in the northern hemisphere, and the short-styled form is the only one described as occurring anywhere around the Mediterranean. Mr. Henslow examined numerous plants in the Maltese Islands and Egypt, and found, with but two exceptions, only the short-styled form. A single plant introduced with some palms observed at Cannes was also short-styled. The exceptions were two long-styled plants in the botanic garden of the School of Medicine at Cairo, one cultivated in a pot, and a stray one in the garden; and these, it is suggested, were a recent introduction from the Cape.

The intercourse in trade which has long existed between Malta

and the African coast, is sufficient explanation of its early introduction into the latter region.

Mr. Henslow goes on to describe the method of multiplication in the northern hemisphere. In Malta the plant is ubiquitous, carpeting the roadsides as well as all the exposed open ground around Valetta, and insinuating itself between the loose stones of the walls, appearing at the surface like a green fringe around each. It covers the tops of the walls in many places, and the lofty fortifications; it forms luxuriant borders to the fields, and also invades cultivated soil, so that, where weeds are not uprooted, a field will look as yellow as an English meadow with buttercups. It is propagated entirely by bulbs. A large plant growing among loose, stony *débris*, if dug up in January, will be found to possess a long tapering stem, throwing off thread-like lateral roots, and bearing minute leaf-scales with small white bulbils at intervals, as well as several larger ones at the crown, below the cluster of leaves. The fine thread-like rhizome extends downwards, sometimes for more than a foot, and grows from a bulb of last season. In many cases when the plant grows as above, the stem proceeds further downwards like a thread; but after a certain distance suddenly increases in diameter, forming a short rod-like structure about 1½ to 2 inches long, with a bulb at the end. This explains how it can reach great depths from which new plants arise in a subsequent season. On plants growing luxuriantly, runners above ground may be formed, which also produce bulbs at the nodes, so that a number of young plants arise at a short distance from the site of the parent.

The author remarks that the extensive vegetative reproduction of *Oxalis cernua* in the Mediterranean, like that of *Elodea Canadensis* in this country, is a strong argument against the assumption of the necessity of cross-fertilisation to ensure a vigorous offspring. Change of hemisphere has also resulted in a changed flowering period, as, in the north *Oxalis cernua* is in full blossom all the winter from November to April, while it is described as flowering at the Cape in June and July. The lengthening of the period is doubtless due, in part, to the fact that no fruit being produced, the plant goes on flowering, as it were, in hope, using up the energy which would otherwise be expended in the formation of fruit and seed.

The Structure of Diatoms.

A recent number of the *Verhandlungen des Naturhist. Med. Vereins zu Heidelberg* (vol. v., part ii.), contains an interesting paper on the structure and nuclear division of Diatoms, by Dr. Lauterborn. Readers of Natural Science (*see* Sept., 1892) will remember the author's name in connection with some researches upon the movement of these organisms which were carried on by him and Professor Bütschli. The present paper is a preliminary communication (with one plate) of a more extensive work which Dr. Lauterborn promises

to bring forward shortly. Besides many interesting details concerning the structure of the protoplasm and shell of Diatoms, and some valuable methods of staining the tissue while living, important and striking results were obtained with regard to the nuclear division, which in these forms always takes place by karyokinesis. The centrosome is always present close beside the nucleus, and visible even during life in the resting cell, but there are no striations round it until the commencement of division. At an early stage in the division there appears, between the centrosome and nucleus, a third body, which is the rudiment (Anlage) of the central spindle—though never spindle-shaped itself. It is probably derived from the centrosome, perhaps by division, but its origin has not been clearly made out. Appearing originally as a rounded body, then becoming rod-shaped, it expands and grows in a peculiar manner, which may best be illustrated by comparing its shape first to a coin, then, by increase of depth, to a piece used in the game of draughts, then to a drum, and, finally, to an elongated cylinder, which is longitudinally striated. Hence, in the rod-like stage its long axis does not correspond to the long axis of the future spindle, but to its transverse axis. When full-grown, it sinks into the nucleus, which, in the meanwhile, has passed through the stages with the dense and loose skein, and has now lost its membrane, and contains V-shaped chromosomes. At this period the centrosome, with its system of rays, becomes indistinct and disappears, and the cell protoplasm shows radiate striations round each of the poles of the central spindle. The chromosomes form an equatorial ring round the central spindle (*Monaster* stage). The ring splits into two rings which travel apart (*Dyaster* stage). The two rings become narrowed and constrict off the two ends of the central spindle, the two pieces so cut off becoming the centrosomes of the daughter cells. The cleavage of the cell passes through the middle of the central spindle, halving it accurately. The two halves of the central spindle perhaps become absorbed into the pieces which were cut off to form the daughter centrosomes. The daughter nuclei reconstitute themselves. The author concludes his paper with some interesting comparisons between this peculiar mode of formation of the nuclear spindle in Diatoms and in other cells. He considers that his observations support Bütschli's view of the homology between the centrosome and the micro-nucleus of Infusoria. It is interesting to note that all the stages of karyokinesis described —and observed chiefly in *Surviella calcarata*, Pfitzer, but confirmed in other species—were seen and drawn in the living state as well as in preparations; also that the protoplasm of these forms shows, both when living and after fixation, a distinct reticulate alveolar meshwork. The striations round the centrosome are seen to be formed by arrangement of the meshes or alveoli in rows, as formerly described by Bütschli. Dr. Lauterborn's complete work will be awaited with considerable interest.

THE principles of geological correlation to which we drew attention in our last number (vol. iii., pp. 404–7), do not seem to find favour with many prominent Fellows of the Geological Society. At the meeting on December 6, one speaker considered that "unless there was a physical change accompanying a faunal one, the latter was of secondary importance. Palæontology must be the servant and not the master of Geology." Another well-known stratigrapher "had no faith in fixing a plane of demarcation between Purbeck and Wealden Beds by means of Ostracoda." Are not these gentlemen somewhat insular in their point of view? They do not seem to distinguish between local and world geology. Lithological differences and other evidences of physical change are, of course, all-important for the local geologist and for mapping, but when it is a question of separating the Jurassic period from the Cretaceous, then some principle is demanded that shall be of rather wider application than the bursting of a river bar, or the closure of a lagoon in our own little corner of Europe. In short, the rock scale is parochial, but the time scale universal.

THE *Fortnightly Review* for December contains the second part of an article by Dr. A. R. Wallace on the "Ice Age and Its Work," dealing with the erosion of lake basins. It is well worth reading as a clear and concise statement of the arguments in favour of the power of ice to cut the basins. One of the points raised seems to be quite new. Dr. Wallace contrasts the well-known indented outlines of submerged valleys, such as now form harbours in the south of England, with the simple outlines of a lake in a glaciated region. In the latter the tributaries fall directly into the lake, instead of flowing into lateral arms formed by the partial submergence of the surrounding country, as would be the case if the main lake originated through subsidence.

WE are glad to hear from letters from Mombasa that Mr. G. F. Scott Elliott left that town for the interior early in November. He has been delayed on the coast in the organisation of his caravan, which numbers 73 porters. The abandonment of the Ruwenzori and Toru Stations will add greatly to his difficulties; but he has left fully resolved to reach his goal of Ruwenzori. We are also glad to hear that Lieutenant von Höhnel has safely arrived at Mombasa. He was seriously injured by a wounded rhinoceros some distance to the north of Kenia, and great anxiety was felt as to whether he would survive the long journey to the coast. He returned *viâ* Kibwezi, where from Dr. Charters his wounds received proper medical attention. His companion, Mr. Astor Chanler, will continue his journey to the north as soon as the men whom he sent back with von Höhnel have returned to him.

I.

Tyndall.

WITH the death of Professor Tyndall has passed away the second of the men whose names are associated as the three English men of science of the Victorian era. His claim to be included in this trio does not rest on his being the deepest thinker, the most accurate and ingenious experimentalist, or the most original investigator in the branch of science upon which he was engaged. If it did so, he might have to yield his place to another, for Lord Kelvin is probably as much his superior as a physicist, as Browning is thought by many to have surpassed Tennyson as a poet. Nevertheless, in both cases, the lesser man may be the more typical of and more influential on the age in which he lived. Though Tyndall's work must be ranked far below that of Darwin, he was far more the representative man of the two, owing to his brilliant versatility, restless energy, his combination of the culture of the literary student with the insight of the scientist and the power of the man of action, his breadth of sympathy and the apostolic zeal with which he fought for a sounder and more scientific system of education.

Professor Tyndall was born in the village of Leighlin Bridge, in Carlow, in 1820, but he came of a Gloucestershire family which is said to have been connected with William Tyndale, the famous Protestant martyr. His parents occupied but a humble position in his native village; his father had been a private in the Royal Irish Constabulary, and after his retirement from this force, he was in business as a small country shopkeeper. Tyndall, however, had the advantage of a fairly good education in one of the small schools which used to be the pride of many an Irish village; here he was well grounded in mathematics and grammar, and acquired from the one subject a habit of scientific method, and from the other an interest in literature. He has himself told us[1] how much he owes to the study of the latter at this school, though he entertained less affection for the discussions on the comparative merits of Popery and Protestantism which occupied a large proportion of his time. In spite of his father's poverty, Tyndall was kept at school till he was nineteen, but this was by no means unusual in Ireland before the days when attendance till thirteen was compulsory. On leaving school he obtained an appoint-

[1] An Address to Students: University College, 1868.

ment as one of the Civil Assistants in the Ordnance Survey, and was at first quartered at Carlow. He was, however, soon moved to Lancashire, and then continued his studies at the evening classes of the Mechanics' Institute at Preston; he at this time intended to become a civil engineer, and contemplated emigrating to America. Some experiments, however, which he saw one evening during a lecture at Preston, roused in him a keen interest in pure science apart from its practical applications.

He remained on the Ordnance Survey for five years and, as he had had experience of most branches of the work, became a well-trained surveyor. When the railway mania burst upon the country, this meant money, and Tyndall in 1844 entered the employment of a Manchester firm; for three years he was kept very hard at work on various surveys, but the excitement and strain and wild unrest of this life were too much for him. He had saved about three hundred pounds, so he threw up his appointment and in 1847 joined the staff of Queenswood College in Hampshire.

This step was probably induced not so much by the fact that the toil of the railway survey was too severe: he had no interest in the work: he was never avaricious and he had already come to feel an enthusiasm for science and had fallen under the influence of Carlyle. He had intended to devote his leisure here to research, but he felt the need of further scientific education before he could seriously commence this. In company, therefore, with his friend and colleague, Dr. Edward Frankland, he left England in 1848.

It was probably due to the teaching of Carlyle that they decided to study in Germany, and there the fame of Bunsen drew them both to Marburg. There they studied chemistry under the great master, and worked at physics under Gerling and Knoblauch. With the latter he wrote one of his earliest papers, "On the Deportment of Crystalline Bodies between the Poles of a Magnet," which was published in the *Philosophical Magazine* for 1850, and was republished in the *Annales de Chemie*, the Archives of the *Bibliothèque Universelle*, and Poggendorf's *Annalen*. At the same time he was a constant reader of Carlyle; and Tyndall has himself told us that it was thus he derived the inspiration which kept him hard at work, and made him face his early morning tub through the cold of a German winter.

From Hesse-Cassell Tyndall moved to Berlin, where he worked at diamagnetism in the laboratory of Professor Magnus, and gained the friendship of Helmholtz and others of the ablest of the younger school of German physicists. He came to London in 1850, and there made the acquaintance of Faraday. Of his first meeting with the man of whom he has often spoken as "my master" he has given us a charming account in his life of Faraday. Immediately on his return to England he received an appointment at the Royal School of Mines, where he found among his colleagues his life-long friend, Professor Huxley. In 1852 he was appointed Professor of Physics at this school, then

in Jermyn Street. The year 1853 was an important one in Tyndall's life. He was elected a Fellow of the Royal Society; and at the instance of Dr. Bence Jones he gave the first of his long series of lectures at the Royal Institution; and shortly afterwards Faraday gained for him the appointment of Professor of Natural Philosophy at the Royal Institution, with which Society he remained connected till his retirement in 1887. For the last 20 years of this period he was, in addition, Superintendent of the Laboratory there, to which office he succeeded on the death of Faraday in 1867.

During his student days in Germany Tyndall made a visit to the Alps, and the impression they made upon him was ineffaceable. He went back, in company with Professor Huxley, in 1856, in order to study some glacial problems, and from that year onwards rarely missed a visit to Switzerland. To his scientific work there reference will be made later on; but no sketch of his life would be complete which did not consider the Alpine feats of which he was so justly proud. He was one of the early group of mountain climbers, and one of the most successful of the band of Englishmen who led the attack on the more dangerous of the giants of the Alps. He must have been a man of splendid physique and sound judgment, or he must inevitably have come to grief during some of his reckless feats. No doubt most of Tyndall's early ascents and glacier expeditions had a scientific purpose; but they were carried out with reckless disregard for life and limb, and he did many things for which he would nowadays get his knuckles rapped pretty briskly in the pages of the *Alpine Journal*. Such, for example, are his ascent of the Jungfrau alone, without a greatcoat, and with only a light alpenstock, which he lost on the top. A still more daring deed was his first ascent of the west point of the Dufour-Spitz—the highest peak of Mte. Rosa—from the Riffel; in the only previous ascent of this point the party consisted of five first-class English climbers, Ulrich Lauener, and three other guides, and they went up, moreover, by the west arête. Tyndall did it absolutely alone.

His greatest Alpine achievement was the first ascent of the Weisshorn from Bies in 1861, in which he was accompanied by Wenger and the old guide who still lives at Zermatt, and has ever since been known as "Weisshorn Bennen." In his attacks on the Matterhorn he was less successful; but for long he held the highest record before the summit was actually reached. He was the third to gain the top, and the first who crossed the mountain from Breil to Zermatt; it was in a ten minutes' rest on the arête during this traverse that he thought out the exquisite little prose poem, "Musings upon the Matterhorn." Three years later he made the third modern traverse of the very difficult glacier pass known as the "Old Weissthor," by a variation on the previous routes, and crossing above the Filar glacier. The last and most difficult of his first ascents was in 1862, when he climbed from Breil with the guides, Bennen, Carrel,

and Walthers, the peak that will long keep alive the memory of his Alpine feats with the name of the "Pic Tyndall."

Most of these ascents, and his other climbing adventures, and he had not a few, were recorded in his charming work, "Hours of Exercise in the Alps" (1871).

Another athletic feat was his expedition at Niagara, when he worked, by the south side of the gorge of the Horseshoe Fall, round the promontory upon which there stood the Terrapin Tower, and under the foot of the Fall.

In addition to his Royal Institution Professorship he held numerous other appointments at different times. In 1855 he was made one of the Examiners under the Council for Military Education. In 1864 he was given the post of Scientific Adviser to the Board of Trade, an office which he resigned in 1883, because he denounced as dishonest the action of the authorities in regard to some new patents. With characteristic generosity he had offered, when he accepted the office, to serve without pay, if his salary could be devoted to the benefit of the sailors. It is needless to say that he received numerous honours from the leading scientific societies at home and abroad; he was awarded the Rumford Medal of the Royal Society in 1864, while he was a doctor of four universities. Halle made him Ph.D., Oxford D.C.L., and both Cambridge and Edinburgh LL.D. In connection with the last it may be remembered that he received the diploma from the hands of Thomas Carlyle, who that year held the office of Rector. With Carlyle Tyndall was very intimate, and he reverenced him as a father. After Mrs. Carlyle's sudden death he carried off the broken-hearted old philosopher to the south of France, and when Froude's publication of her letters had thrown a shadow on the fame of Carlyle, no one stood up more manfully than Tyndall in the defence of his old teacher and friend. The bust of Carlyle on the Chelsea embankment was unveiled by Tyndall about this period, and the prophecy at the close of his speech was one of those that greatly help their own fulfilment. He compared Carlyle in nobility of form and ruggedness of strength to the Bel Alp that rises at the back of his châlet on the Eggishorn, and the shadow that overhung the memory of Carlyle to the bucket of water whipped up into a cloud that could temporarily obscure the mountain outline; but he predicted that the mist would soon be swept away, and the character of Carlyle stand out as clear and spotless as before.

Tyndall's connection with the British Association is mainly recollected owing to his famous Belfast address in 1874, in which he discussed the relations of contemporary scientific thought and revealed religion; the orthodox party took alarm at the views therein expressed being proclaimed so publicly before the world; and Tyndall was pelted with pamphlets, scolded in sermons, and denounced in the press. The address was unquestionably a materialistic manifesto, and it is a remarkable sign of the progress of the last twenty years, that

those who, like *The Times*, denounced it at the time, accept it to-day. Tyndall first went to the meetings of the Association at Ipswich in 1851, and for some years was a regular attendant; in 1867 he delivered the "Working Men's Lecture," taking as his subject, "Matter and Force"; he was President of the Mathematical and Physical Section in 1868, when his address dealt with "Scientific Materialism." In 1870 he was the lecturer for the year, and read his famous essay on the "Scientific use of the Imagination."

In later years Tyndall was not so prominently before the public, especially after his marriage, in 1876, to the Hon. Louisa Hamilton, the eldest daughter of the late Lord Claud Hamilton. He spent most of his time at his house at Haslemere, and went every summer to his chalet, the Villa Lusgen, in Switzerland. In 1877, however, he accepted the Presidency of the Birmingham and Midland Institute, and gave as his inaugural address a lecture on "Science and Man," by which he again roused the wrath of his theological opponents. In 1887, he resigned his position at the Royal Institution, and was entertained at a farewell dinner by what was described by *Nature* at the time as the most representative gathering of English Science that had ever been held. Since then he had written or lectured but little, though the Orange blood that was in him led him to enter into political warfare in opposition to the different Home Rule schemes. Two years ago he was taken dangerously ill, but he recovered, and he visited Switzerland this summer as was his wont. He returned in October, and, in consequence of a tragic accident, peacefully passed away on the 5th December.

It is difficult for one who is not a physicist to give any sketch of Tyndall's work in science, as his main work lay in this department of research; but this I may be allowed to attempt, as physics do not come within the domain of NATURAL SCIENCE. The first research was upon "The Phenomena of a Water Jet," which was not, however, published till 1851. The subject which he made especially his own was diamagnetism, upon which he wrote about twenty papers, commencing with a short note "On Diamagnetic and Magnecrystallic Action," published in 1850 in the second volume of the *Chemist*, and in the Reports of the British Association in the following year a more detailed memoir on the same subject, entitled "Diamagnetism, the polarity of the diamagnetic force, the magneto-optical properties of crystals, and the relations of magnetism and diamagnetism to molecular arrangement"; this gained for him admission to the Royal Society. Radiation was another question on which he did much work. He took it as the subject of his "Bakerian" and "Rede" Lectures in 1864 and 1865, when he announced his discovery of "calorescence," in which, by altering the refrangibility of the ultra-red rays of the spectrum, they can be rendered visible. This is simply the converse of Stokes's well-known experiments on "fluorescence," by which the ultra-violet rays, after analogous treatment,

can be seen. Some of his most interesting results with radiation were in connection with its meteorological influence. Thus he showed that the effects ascribed by de Saussure, Hopkins, and others to the atmosphere, really result from the aqueous vapour in it, which forms the blanket protecting the earth from excessive cold at night. His study of the atmosphere in this connection subsequently came in usefully during his elaborate experiments on fog-signals. In relation to the same subject, he was led to study the effects of the floating particles in the air, and he showed that the blue of the sky, of the Alpine lakes, and of the sea is due to the polarisation caused by these particles. He manufactured artificial sky by some new chemical reactions produced by light, in order to demonstrate the truth of these conclusions. Another outcome from his interest in this line of inquiry was his work on the putrefactive influence of the floating particles of the air. This he summarised in two lectures; one on "Fermentation, and its bearings on Surgery and Medicine," delivered before the "Glasgow Science Lectures Association" in 1876, and one on "Dust and Disease," in the course of which he described a fireman's respirator made from cotton wool and glycerine which he had invented.

More important scientifically was his valuable investigation into Dr. Bastian's claims to the discovery of the spontaneous generation of bacteria in vegetable infusions. As Tyndall has told us, as a believer in the nebular hypothesis, he is logically bound to believe that spontaneous generation did come about some time; but with true scientific candour he clearly showed that Dr. Bastian's experiments had not been performed with sufficient care, and that when full precautions are taken, the bacteria do not appear in the infusions.

His geological work is far more important than his biological. His first contribution to this subject was a lecture "On some of the Eruptive Phenomena of Iceland," delivered before the Royal Institution in 1853, in which he illustrated by experiments the causes of Geysirs. His remaining geological papers may be divided into two groups, first, those bearing on slaty cleavage; and second, on glaciers and glaciation. His main contribution to the former was published in three papers in 1856; one was "Observations on the Theory of the Origin of Slaty Cleavage of H. C. Sorby" (*Phil. Mag.*, vol. xii., pp. 161-164), the second was a lecture on "Crystalline and Slaty Cleavage," finally published in his "Fragments of Science," and the third was a "Comparative View of the Cleavage of Crystalline and Slaty Rocks" (*Proc. R. Inst.*, vol. ii., pp. 295-308). In these three papers he agreed with Sorby as to the fundamental difference between the mechanical and crystalline cleavage, and annihilated Professor Sedgwick's view that slaty cleavage is identical with crystalline, and that therefore mountains composed of slate are really often one huge crystal.

But Tyndall went further than Sorby and showed that slaty cleavage can be induced by pressure in an apparently homogeneous

body, whereas in the experiments of the latter some flaky particles such as mica were always mixed with the substance used. Later on, however, Tyndall recognised that the lateral yielding of the clay under pressure, and the consequent tangential sliding of one particle over another, contributed very largely to the result.

Tyndall's glacier work was more extensive and important. His main contribution was the discovery of regelation, by which the *method* of glacier motion is easily intelligible. He next showed, by some difficult work on the Mer de Glace in mid-winter, that the glaciers move in the winter as well as in the summer, and determined the rate. He subsequently showed how the well-known "veined structure" of glaciers is a result of the flow of the ice. His principal paper upon the subject was one contributed to the *Philosophical Transactions* in conjunction with Professor Huxley in 1857.

His studies upon this subject were collected and summarised in his books on the "Glaciers of the Alps," and in "The Forms of Water," which were published in 1860 and 1872 respectively. In addition to his work on the properties of glacier ice, he closely watched the action of the glaciers and held somewhat extreme views as to the extent of their erosive power. Thus he assigned a far larger influence to glacial erosion in the configuration of the Alps, in his lecture in 1864, than is fashionable at the present day. He joined in the controversy on the origin of the Parallel Roads of Glen Roy, and advocated and restated with great clearness Jamieson's theory of their formation by the action of a glacier dam.

It is, however, less on his work as an original discoverer than in his brilliant powers of lucid exposition that Tyndall's influence has depended. It is generally argued that lecturing was his forte, and in this he was unusually successful. Most of his text-books were based on his lectures, such as his "Lessons in Electricity," which were given as a course of lectures to children at the Royal Institution, and show his capacity for using the simplest of apparatus. His books on "Sound," "Light," and "Heat, considered as a mode of Motion" were also compiled from lectures. The two first were given in a tour in the United States in 1872, the profits of which, amounting to 13,000 dols., he presented to the Universities of Harvard, Columbia, and Pennsylvania, for the endowment of research scholarships.

The department of this work for which, perhaps, the younger generation owes Tyndall the deepest debt of gratitude, is that, in conjunction with his two friends, Herbert Spencer and Huxley, he fought the fight which gained for Natural Science a free hand in the investigation of the whole range of natural phenomena. This now sounds like a truism; but it was not when he wrote that "the impregnable position of science may be described in a few words. We claim, and we shall wrest from theology, the entire domain of cosmological theory. All schemes and systems which thus infringe upon the domain of Science, must, in so far as they do this, submit to its

control, and relinquish all thought of controlling it." If a distinguished prelate were to stand up before the British Association to-day, and say that recent theories on the sequence of Plutonic rocks did not agree with the order of the strata assigned to the Holy City in Revelations, and were therefore unorthodox, and therefore necessarily wrong, he would be laughed at. But when Bishop Wilberforce denounced Darwinism in 1857 this sort of argument was valid, and had to be replied to. The fact that the same is not the case now, is no doubt largely due to the Agnostic crusade of Professor Tyndall. It is now universally recognised that in all matters of natural phenomena and physical energy, Science is the final court of appeal, and that if religion teaches differently so much the worse for religion; the very reference to prayer as a form of physical energy, which was accepted 40 years back, now sounds irreverent, so great has been the progress.

During the course of this fight Tyndall made for himself many foes, in addition to those such as Forbes, Tait, and Ericson, whose enmity he had aroused during the course of his scientific work. Thus his series of papers on prayer and miracles, and his proposal to test the former by a series of laboratory experiments on a large scale, roused the fighting section of the Church. When Oxford gave him the degree of D.C.L., it was in face of the protest of some of the "old school." It was doubtless to the same cause that he owed the caricature of him that appeared in "The New Paul and Virginia," where as "The Professor" he appears as the vulgarest and coarsest of the creations of Mr. Mallock's usually subtle spite.

A second belief which Tyndall has largely helped to impress upon the public mind, is that the value of scientific education is not the dissemination of a mere catalogue of facts, but of more precise methods of thought. As he put it, "science should be studied not as a branch of education, but as a *means* of education;" we are therefore bound to carry the scientific method into every department of thought, and work, and life, and to carry every argument to its logical conclusion, let them clash with whatever cherished beliefs they may. When Tyndall returned to England from Germany this was not the current method of thought. Men worked at science, but feared its conclusions; it was the date when, as Tyndall has put it, there were "Tories even in science who regard Imagination as a faculty to be feared and avoided, rather than employed." There were men like Faraday who seemed to belong to a kind of "Manchester School" in science, who worked at science all the week, and preached Sandemanianism on Sundays: they divided their brains into two watertight compartments, and put their reason in one, and their religion in another, and trusted to habit, fashion, and indolence to keep the bulkhead impervious. But Tyndall had no sympathy with this school of thought: he believed that, on the other hand, "science has of late years assumed a momentous position in the world. Both in a material,

and in an intellectual point of view, it has produced, and it is destined to produce, immense changes—vast social ameliorations, and vast alterations in the popular conception of the origin, rule, and government of natural things." He therefore strove with a missionary zeal to shatter this dividing wall between science and general thought; and he did it. It was perhaps his recognition of a certain analogy between this bisected brain and the political brain of a later date, which often appears to be divided by a double bulkhead into three separate compartments for politics, morality, and reason, that led to the vigour of his anti-Gladstonian utterances. He recognised an old foe, and promptly beat his pen into a tomahawk, and dashed into the fray yelling the fiery warwhoops of his race.

There was no doctrine, he thought, savoured more of the evil one than this system of purchasing intellectual peace at the price of intellectual death; and throughout the whole of his career, in spite of all opposition and abuse, he hurled against it his most powerful ridicule and invective, and gave expression to the most passionate appeals that he could utter. The world has many such refuges, he said, and they will be used by all "to whom repose is sweeter than the truth. But I would exhort you to refuse the offered shelter, and to scorn the base repose; to accept if the choice be forced upon you, commotion before stagnation, the breezy leap of the torrent before the fœtid stillness of the swamp."

Thanks to his lucidity of exposition and the fascinating charm of his clear and nervous English, his efforts have exercised a deep influence, and before his death he had the pleasure of seeing the gospel he had preached from presidential chairs, in lectures, essays, and the Press accepted by the majority of his fellow countrymen. So much so has this been the case that even in reference to the Belfast address itself *The Times* could say on his death, "we all stand to-day where Tyndall stood 20 years ago."

It was, perhaps, an even greater satisfaction to him that he could watch the improvement in scientific education, for which he had so long contended, and the wider interest in which he did so much to foster. When we recollect how he was ever ready to leave the quiet of his study to help on the spread of scientific thought among his countrymen, one can but think that he had taken as his life motto the words of the old Israelitish leader that we may now adopt as his epitaph:—"Speak to the people, that they go forward."

J. W. GREGORY.

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II.

Natural Science in Japan.

I.—Past.

FEW people in this world have the love of Nature so strongly inborn in them as have the Japanese. It pervades all their life. Religion, art, poetry, daily pursuits and holiday recreations: all are imbued with this nature-spirit, that seems to belong to this nation like it has to no other with which the western world has been acquainted since the days of ancient Greece.

Pardon me, severely scientific readers, while I illustrate these bold remarks more fully.

Shinto, "the way of the Gods," which, before the introduction of Buddhism from China, served the Japanese as a religion, and which the restored Mikado has of late endeavoured to re-instate in its former pre-eminence, is a religion, not so much in our modern moral acceptation of the term, as in the primitive meaning that it had for the Romans of old. A veneration of ancestors; a worship of all the powers of nature, of wind and ocean, sun and fire, food and pestilence; a worship mingled with love that has personified the rivers, the mountains, and the trees of their beautiful native land; a religion of artless forms and homely prayers, that eschews both the maxims of the "ower guid" and the sermons of the "ower lang," and that is almost pathetic in its natural simplicity: such is Shinto.

Art, not the conventions of the schools and the copies of Chinese masters, but the living naturalism started by Okyo and perfected by Hokusai—how it sports with and idealises the minutest facts of this natural world! Natural this art is in its irregularity, even in its impossibility, and above all in the life with which it is instinct. It is not the remote or the grand that inspires these artists, but the near, the quaint, and the beautiful. "The Japanese," one must admit with Basil Hall Chamberlain, "are undoubtedly Raphaels of fishes, and insects, and flowers, and bamboo-stems swaying in the breeze." Not merely the painters, but the carvers in wood and in ivory, the workers in clay and in metal, all are caught in the embrace of an ardent naturalism chastened by a pure simplicity. With what loving diligence the craftsman toils over his golden representation of a beetle, with wings, elytra, and mouth-parts all complete, while the patience of the sculptor who gave five years to making the

feathered eagle of steel, lately shown at Chicago, is hardly to be conceived by us hurried Europeans.

Poetry, with all its conventions, and they are neither few nor easy, is for the Japanese almost entirely an impressionist rendering of nature. Sometimes a mere sketch, that appeals to one solely by its beauty; sometimes a piece of such sympathy with the soul of things that its subtlety and delicacy perpetually charm while they perpetually elude us. But this is a subject on which my pen is apt to run away with me, so in this place I must content myself with translating, however imperfectly, a poem of either kind. First, then, read this description of—

AN AUTUMN EVE.

Passed the shower; but yet
On dark fir-leaf its memory lies
In pearls of glistening wet,
Wherefrom silver mists arise
Veiling in night the autumn skies.

And now hear this, remembering that it is centuries older than the song of Keats:—

MORNING-GLORIES.

Shame! that one should call
Morning-glories' dew-sprent gleam
A thing ephemeral.
Flowers, on that wise, might deem
Man an evanescent dream.

Thus we pass to the pleasures and pursuits of the people, of which this last poem inevitably reminds us. For if there is one thing characteristic of the Japanese, it is their love of flowers. When I was in Tokyo it was late summer, and early every day the florists' gardens at Irya were thronged by those who came to see the morning-glories bloom, while towards evening worshippers visited the island shrines of Benten to delight in the sacred lotos that flowered around them. Everyone has heard of the numbers that crowd that wonderful avenue at Ueno Park when its double cherry-blossoms float like pink clouds in the air, while the world itself has yielded to the chrysanthemum, the royal symbol of Japan. Nor is this merely fashion, of the same sort that takes some of us to private views and musical "at homes." No house in Japan so small but has its little landscape garden, if only in the back-yard; and no inn so humble but can find a flower to brighten the room of every guest.

The love of animals, though not perhaps quite so conspicuous, is nevertheless sufficiently obvious. Some of this may be due to the influence of the Buddhist religion with its principle of incarnation. This at least places a great check on the taking of animal life, so long, that is, as the animals are warm-blooded. This too, together with various mythological traditions, may account for the prevalent practice of keeping sacred animals attached to the various temples. These animals are generally albinos, but also include herds of deer,

tortoises, and carp. In a few cases the number and different kinds of animals give the temple-yard almost the aspect of a zoological garden, and the resemblance is by no means diminished by the light-hearted crowds that pass from cage to cage, feeding the inmates with biscuits and beans. Whatever may be the theological explanation of this, the mere fact argues an initial sympathy with the brute creation, of that peculiar but common kind that manifests affection by imprisoning its object. Primitive and child-like this spirit may be; but we know it well enough ourselves, and so we have a fellow-feeling for these bright-eyed, brightly-clad little damsels that fly their butterflies fastened to a long black hair, and these laughing troops of boys that chase the cockchafers with long bamboo poles sticky at the tip.

More need not be said. The love that these children of nature have for their mother is now abundantly exemplified. But this feeling, as my friend O. H. Latter has recently reminded us (NATURAL SCIENCE, vol. iii., p. 41), is in no way connected with the scientific spirit. The demand for natural history objects supports more than one shop in Tokyo, itinerant vendors of the same ply a thriving trade, and the travelling raree-show reaps a harvest in every town. But the objects sold and exhibited are such as please children, not men; they excite curiosity and wonder without conveying knowledge.

At last, however, the scientific spirit, with other such modern improvements as quick-firing guns and labour disputes, has invaded the land of the rising sun; and it is the object of this paper partly to trace its gradual growth, partly to show how it flourishes to-day, now that the Japanese nation have cast off the leading-strings that they borrowed for a time from the Western world.

Just as, in ancient times, Japan received from China and Corea her religion, her literature, her art, and such science as there was to receive, so at the present day she has, for good or ill, adopted almost wholesale the methods and results of our own vaunted civilisation. The modernisation or Europeanisation of Japan has been a purely defensive measure, initiated and carried on by a certain section of the Japanese themselves, and notably by their great statesmen, the Counts Ito and Inouye. But the actual working out of the scheme has of necessity been placed till recently in the hands of foreigners employed by the Japanese Government. In this development two great races, the Anglo-Saxon and the Germanic, have played prominent parts, and in the particular parts they have played the respective genii of these two races have conspicuously asserted themselves. The English-speakers have, as usual, devoted themselves to the more practical and obviously utilitarian side of life, embellished, as usual, by the attempt to impose on an alien people their own particular religious opinions. To them are due the railways and the College of Engineering, the Navy and the Mint, the Press and the Prayer Book. The Germans, on the other hand,

have exerted their influence chiefly in the direction of history, medicine, and the natural sciences. It is to the labours of such men as Engelbrecht Kaempfer, C. P. Thunberg, P. F. von Siebold, Edmund Naumann and Max Fesca that the present position of natural science in Japan is largely due. Of late, too, many Japanese students have been attracted to America rather than Europe, both by the improvement of scientific education there and by the greater nearness of that country. As a consequence of all this, English scientific men have less communication with their Japanese colleagues, and know less about their doings than it would be to their advantage to have and know. And this it was that suggested the present account.

The present state of Japanese science will be better appreciated if we compare it with what has preceded, and trace its gradual development.

It was originally through the commercial enterprise of the Dutch that scientific men were able to acquire for themselves some knowledge of this far country, and at the same time to introduce to its inhabitants some of their own more advanced methods and knowledge. The *materia medica* of snakes' skins, dragon's blood and more disgusting, if less rare, substances, and the oriental nostrums of massage, acupuncture and moxa were, if not superseded, at all events modified by the wider knowledge of drugs and the more rational acquaintance with human anatomy communicated by the physicians of the Dutch East India Company. Of these men the first of importance was Kaempfer, a Westphalian, who came to Nagasaki, then the only port of Japan with which the Dutch were permitted to trade, in 1651, being then 39 years of age. He stayed in Japan two years and two months, during which time he was, like other foreigners, practically confined to Deshima, an island district of Nagasaki, except on the two occasions when he attended the Dutch Embassy on its yearly visit of homage to the Shogun's Court at Yedo, now Tokio. In spite of the disadvantages under which he laboured, he collected a vast amount of information, subsequently given to the world in his *Amœnitatum Exoticarum Politico-physico-medicarum Fasciculus V.* (Lemgo, 1712), and in the *History of Japan*. Here we may venture to pride ourselves on the fact that at least the original publication of the latter work was due to an Englishman, Sir Hans Sloane, who purchased the manuscript, had it translated into English by Dr. J. G. Scheuchzer, and published the translation at London in 1727-8; and this English version was the basis of all subsequent foreign editions of the work.

In Kaempfer's time a knowledge of natural history seems to have been very sparingly diffused among the Japanese. Monsters, indeed, they were well acquainted with, such as the unicorn, by them called Kirin, the Hoō, which is generally translated Phœnix, and the sea-dragon or Ryō; but animals of a less exalted type appealed

to them in a very small degree. Kaempfer regrets that the shells and other marine invertebrates, such beautiful material for a zoologist, are wasted on the sea-god, whom he calls Jebus. Pearls, indeed, they prized, having gained from the Chinese a knowledge of their value; but they knew so little of their nature as to imagine that some of them could grow of themselves and breed fresh pearls. Earthquakes, frequent though they were, they still ascribed to some large whale burrowing under the islands. We cannot, however, venture to laugh at the Japanese, for the works of Gesner and Aldrovandus, published not half a century before, contained monsters quite as terrible and myths no less absurd.

Over a century elapsed before another man of any scientific importance found his way to Japan. At last, in August, 1775, C. P. Thunberg, the Swedish entomologist, came as physician to the Dutch legation, and he stayed in Nagasaki till December, 1776. He was not idle during his visit, and the results are to be found in his *Flora Japonica*, published at Leipzig in 1784, in *Icones plantarum japonicarum*, five folios published at Upsala, 1794-1805, and in two volumes issued at Paris in 1796 and entitled *Voyages de C. P. Thunberg au Japon*. The natural history portion of the last work is comprised in chapters xviii., xix., xx., and xxiii., of volume ii. Besides making researches on his own account, Thunberg instructed some Japanese in various branches of natural history, especially botany.

Again we pass over three-quarters of a century before the arrival of the greatest German of all who have visited Japan. Philipp Franz von Siebold was born at Würzburg in 1796. In 1822 he entered the service of the King of the Netherlands as physician to the East Indian army, and on his arrival at Batavia was appointed leader of a Dutch scientific mission then starting for Japan, in which position he landed at Nagasaki in August, 1823. Here, as Professor Chamberlain writes in his useful book *Things Japanese*,—"By force of character, by urbanity of manner, by skill as a physician, even by a system of bribery which fell in with the customs of the country, and which surely, under the circumstance, no sensible man of the world will condemn, he obtained an extraordinary hold over the Japanese, suspicious and intractable as they then were. Having, in 1826, accompanied the Dutch embassy to Yedo, Siebold obtained permission to remain behind—the sole European in that great Asiatic capital, then absolutely sealed against the outer world. The excuse pleaded and accepted was that he would instruct the Japanese physicians in the more recondite branches of their art. His leisure he utilised in multifarious scientific researches; and so well did he know how to ingratiate himself that some of the highest in the land willingly added to his store of knowledge. Suddenly a rumour got about that the chief Court spy had sold him a map of the country. This was treason according to the old Japanese law. The spy was ordered to commit *harakiri* [disembowelment], and Siebold

was cast into a dungeon, from which he emerged only on the 18th January, 1830, with strict orders never to return to Japan." In 1859, however, after the American, Commodore Perry, had forced open the gates of Japan, Von Siebold returned as a semi-official ambassador, and, for a time, actually entered the Japanese service as a negotiator. In these troublous years he was not so successful, either in politics or science, and in 1862 he returned to his own country, where he died four years later.

The best known of Von Siebold's works is *Nippon: Archiv zur Beschreibung von Japan und dessen Neben- und Schutzländern*, a splendidly illustrated folio, published at Leyden and Leipzig, from 1834 to 1842, with the assistance of the King of the Netherlands. In conjunction with C. J. Temminck, H. Schlegel, and W. de Haan, he published a *Fauna Japonica*, which appeared at various dates between 1834 and 1851; while, with the assistance of J. G. Zuccarini, he presented to the Royal Bavarian Academy of Science a memoir entitled *Floræ Japonicæ familiæ naturales*. It is not, however, any of these elaborate works that now demands our attention, but, rather, a rare and little-known quarto pamphlet of 16 pages, originally published in 1824 at Leyden, and fortunately, since none of the London libraries seem to possess it, afterwards reprinted in Oken's *Isis*, Bd. xx., Heft 2, columns 135-143, 1827. Its title is *De historiæ naturalis in Japonia statu, nec non de augmento emolumentisque in decursu perscrutationum expectandis dissertatio, cui accedunt spicilegia faunæ Japonicæ*, and it contains so very curious and interesting an account of the state of natural science in Japan at the time of his visit that, considering its general inaccessibility, I think I cannot do better than quote from it somewhat extensively.

In this account of Von Siebold's, among other characteristics of the Japanese, some of which have already been alluded to, we shall notice especially these three: their love of the monstrous, the keen eye united with the cunning hand, and the practical bent of their minds. We shall further notice that the position of natural science had considerably improved since the days of Kaempfer, 175 years before. In fact Kaempfer's own labours, followed by the example of Thunberg, had aroused among the learned Japanese an almost incredible ardour and curiosity in natural history.

Botany, says Von Siebold, was especially cultivated by them, partly for its service in the pharmacopeia, since, like all oriental nations, they were much given to herbs, and had at least 500 in daily medical use, partly since they depended on the vegetable kingdom for almost all the necessaries of life, in the way both of food and clothing. But not entirely to these practical ends did they study the science, since our author adds, "for their own pleasure and for the ornament of their houses they cultivate the rarer plants, which they are at pains to procure not only from distant provinces of their own land, but even from China, and especially from Corea." Their

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botanical knowledge came originally from China, and so they denoted not only these introduced plants, but also the common native ones, by Chinese characters. From Thunberg, however, they had also received the Linnean names, and remembered them with pleasure. In their rare intercourse with either botanists or their books, it was hardly to be expected that they should have made a systematic study of botany; "nevertheless," says Von Siebold, "I have often wondered at the exactness and dexterity with which they distinguish the slightly differing species of a complicated genus and define their varieties." And, again, he tells us how the Japanese paint flowers, both wild and cultivated, so accurately that one can easily distinguish genera and species. A beautiful example of a Japanese botanical work of this period is the *Honzo Zufu*, by Iwasaki Tsunemasa, Yedo, 1828, of which a copy, belonging to Mr. A. Bisset, is deposited in the library of the Botanical Department of the British Museum.

The study of Zoology had prospered less; for investigations into the qualities of animals seemed to this herbivorous nation less necessary and useful than into those of plants. "At the same time," says Von Siebold, "they have a very accurate knowledge of such animals as do happen to be of any use to them, especially of fish, crustaceans, shell-fish, and certain other molluscs. They often form collections of shells, and make various articles out of them. In all their collections they value a thing the more for its montrosity. They carefully preserve all deformities and oddities that Nature has chanced to issue from her mint, and what Nature is unable or unwilling to produce, they manufacture by art." The Mermaids and Dragons composed of parts of different animals joined by *papier maché*, such as are still to be seen in the museums attached to certain temples, are described by Von Siebold, who admits that they so rivalled nature as easily to deceive the eyes of a tyro. "They have even," he exclaims, "attempted to deceive me with a human fœtus made monstrous by their craft. Frogs and tortoises with supernumerary heads or limbs they esteem as rarities. The vogue of these artifices is shown by the fact that they are exposed for sale at exorbitant prices. The love of lucre invents fresh monsters from day to day, and is a strange stimulus to zoological labours. To this end they keep deer, bears and monkeys, also white rats, hares and rabbits, and they rear the rarer birds which they collect from remote provinces and even from China. Although the works of Linnæus may be found here and there, chiefly in the city of Yedo, and though his method of description may in some cases have been followed by the Japanese, nevertheless I have never found traces of any systematic zoology, while the pictures of many mammals, said to be native in the interior of Japan, display nothing characteristic, although at a first glance you would have called them well painted, since their colouring is true to life. These same drawings of the animal

kingdom furnish another example of the peculiar mental attitude of the Japanese, since they fashion for themselves with their brush many birds and butterflies that are certainly not native nor have ever been seen. Still I admire their finer pictures of fish and butterflies drawn according to nature."

Geology is next treated of by Von Siebold. "I observed," he writes, "that the medical men whom I met daily could well distinguish the common minerals, such as Antimony and Zinc, and define them by their Dutch, Japanese, and even Chinese names. But in private collections of minerals, I have noticed, as above, that they chiefly esteem and preserve monstrosities. Hence fossil animals, fossil plants, with other stones curiously shaped by nature or chance, form the rarities of their collections." Obviously, though the Japanese collected fossils, they were not palæontologists; they even confused fragments of corals with simple stones. That they were excellent metallurgists, however, the fame of their copper, iron, brass, silver, gold, and gold alloy, is sufficient proof. They were also not ignorant of how to treat minerals by chemical operations, and made many preparations of mercury.

In conclusion, Von Siebold emphasises the desire of the Japanese for knowledge and their aptitude for investigation. He founded a medical college (which must have been in Nagasaki before he went to Tokio) and employed his students collecting specimens. Among interpreters and other natives he found "a liking and singular ability for natural history, excited partly by scientific, partly by pecuniary reasons." "In two weeks," he says, "I taught a Japanese youth the art of drying plants and all the methods of taxidermy, so that every day I more and more admired his manner of preparing specimens." Lastly, the more learned Japanese "lose no opportunity of gaining knowledge from European books."

My readers have by this time, if my attempt has been successful, gained some idea of the ground in which, when at last the fence was broken down, the seed of Western science was to be sown. And by this time they will be getting anxious to learn how the plant has flourished and what fruit it bears at the present day. This it will be the main object of the second portion of this paper to inform them.

F. A. BATHER.

(To be continued.)

III.

The La Plata Museum.

AS many of our readers are probably aware, when the city of Buenos Aires was made the capital of the Argentine Republic in the year 1880, it was at the same time considered advisable that the province of this name should have a capital of its own. Accordingly, two years later, the city of La Plata was founded on a site some thirty-five miles lower down the river than the metropolis, and inaugurated as the provincial capital of Buenos Aires. One of the reasons for selecting a site so near to the metropolis appears to have been the necessity of having a port where vessels of the largest tonnage could come alongside the wharves; and one of the first proceedings was to construct a magnificent ship-canal from the river, terminating at a spot known as the Ensenada, within easy distance of the centre of the new city, and provided with accommodation for ocean steamers of almost any dimensions. With characteristic Transatlantic energy, the planning and building of the future capital was pushed on apace; and there soon arose on the site of what had recently been a mere *estancia*, or cattle-farm, a city of lordly palaces and stately squares, which has been not inaptly designated the "enchanted city." While the "boom" which at that time was at its zenith in Argentina lasted, La Plata bid fair to become a rival of the metropolis, and population and building vied with each other in trying to get ahead. Unfortunately, these rosy prospects were but short-lived, and at the present time the princely palaces and broad boulevards of the "enchanted city" are, except during the sittings of the Provincial Parliament, well-nigh deserted; and instead of resounding with the rattle of carriages and the tramp of thronging multitudes, the paved streets are silent, deserted, and grass-grown.

Whether this state of comparative desertion and stagnation is likely to be permanent, or whether it is but the chrysalis-stage preparatory to the advent of a period of prosperity and progress, it behoves us not to enquire in these pages. There is, however, within the limits of La Plata, a noble building which must for ever render celebrated the name of that city throughout the length and breadth of the scientific world. This building, I need hardly say, is the Museum, which owes its foundation and present prosperity solely to the indomitable energy and perseverance of its able and accomplished Director, Dr. F. P. Moreno.

Recognising the importance of a proper appreciation of science in a country so richly endowed with palæontological treasures, Dr. Moreno lost no time in impressing on the Provincial Government the necessity of providing funds for the erection of a Museum which should be worthy of the palatial surroundings of the enchanted city; and, fortunately for science, his efforts were ere long crowned with the success they so well merited. Only two years after the inauguration of La Plata as a city—namely, in 1884—Government sanction was obtained for the erection of the Museum; and in 1889 the imposing edifice represented in our first illustration was practically complete. The building, we may state, is situated on the Ensenada, or river-side of the outskirts of the city, standing in a park, amid splendid avenues and groves of tall eucalyptus and other trees, which, in the course of a few years, will form a veritable forest. Since our excellent illustration (for which, like the others in this notice, we are indebted to Dr. Moreno) gives such a good general idea of the external appearance of the Museum, it will not be necessary to say much on this point; neither shall we spend much time on a description of the interior. We may mention, however, that after passing under the well-proportioned Grecian portico, the visitor, on entering the building, finds himself in a rotunda, with a gallery and roof supported by two tiers of iron columns, and lighted above by a large skylight; its walls being decorated with frescoes representing the scenery, native life, and some of the wonderful extinct mammals of Argentina. From this rotunda, which occupies the centre of the front of the building, there diverge, on the ground floor, two galleries on opposite sides, which, after running a straight course for some distance, curve round so as to form a pair of apses at the two extremities, which are again connected by a straight gallery running parallel to the one in front, both back and front galleries being connected by cross-galleries and chambers, so that the whole edifice forms a continuous block of building. The upper floor, which does not extend over the two terminal apses, contains the apartments of the Director and the Secretariat, together with the library, the art-gallery, and some portions of the ethnological section. On the ground-floor the central chambers are, in the main, devoted to anthropology and ethnology; while the galleries on the right of the entrance contain the geological and palæontological exhibits, and those on the opposite side the animals of the present epoch. The central hall, on the further side of the building opposite the rotunda, is, however, consecrated to the mastodons and sub-fossil cetaceans.

We might, of course, elaborate to any extent the description of the Museum itself, but since this is a matter of comparatively small interest, we proceed at once to the consideration of its contents. And here we may state at the outset that the most marvellous thing connected with this wonderful institution is the circumstance that nearly the whole of its unrivalled treasures have been collected within

the last few years by the untiring energy of its Director. Probably many persons in England are under the impression that the La Plata Museum and the Buenos Aires Museum (of which the palæontological contents were so admirably described by its late Director, Dr. Hermann Burmeister) are one and the same thing. This, however, is far from being the case, the Buenos Aires Museum being the National institution, while the La Plata Museum pertains to the province only. Without entering into the question whether or no the establishment of two such institutions within forty miles of one another was strictly advisable in the interests of science, we may state that at the foundation of the La Plata Museum, the National Museum at Buenos Aires was left untouched, and the collection of the former started almost *de novo*. And we may add, in no disparagement to the elder institution—which must always claim the prestige of containing the whole of Burmeister's types—that the younger sister has shot far ahead, so far as palæontological treasures are concerned.

So far as we can gather from an account published by the Director in 1890, it would appear that the principal aim of the Museum is to illustrate the whole fauna—both recent and fossil—of the Argentine Republic. It has, however, been recognised that it would be impossible to study the unrivalled series of fossil mammals without the opportunity of comparison with the skeletons of the living members of the same class from all parts of the world; and the Director has accordingly paid special attention to the acquisition of a representative series of specimens of mammalian osteology; our second plate showing the gallery where the greater number of these skeletons are exhibited. Among those of more than ordinary interest are the fine series of skeletons of South American Cetaceans, most of which have been obtained from the estuary of the Rio de la Plata and the coast of Patagonia, where several of them have been found stranded. The series includes skeletons of *Balænoptera*, *Megaptera*, *Hyperoödon*, and *Orca*, most of which have been referred to species distinct from those of the northern hemisphere; and among these the splendid skeleton of a member of the first-named genus, seen suspended on the left side of our plate, is remarkable for its large size, the total length being upwards of 22½ metres. Of still wider interest is the skeleton of an individual of *Neobalæna marginata*—a cetacean which we believe to have been hitherto recorded only from the South Seas. With the bare mention that a large number of both native and foreign animals are represented by stuffed specimens, we must bring to a close these few remarks relating to the recent section of the Museum, and proceed to the palæontological department, which is the one from which the institution will derive a world-wide celebrity.

Before setting out on my recent visit to La Plata, I had been prepared by the glowing accounts sent to me by the Director, as well as from the published writings of other palæontologists, to find the

Museum exceedingly rich in the fossil Vertebrates of Argentina; but on my arrival the reality far exceeded my most eager expectations, and during my first walk through the seemingly endless galleries of the Museum I was absolutely lost in astonishment and admiration at the number and beauty of its palæontological treasures. We find, for instance, in one of the galleries devoted to the display of the mammalian remains from the Pampean beds and the somewhat older formations of Monte Hermoso, near Bahia Blanca, two complete skeletons of *Toxodon*, while another of *Macrauchenia* lacks only a few of the hinder trunk-vertebræ. The latter genus is also represented by three complete mounted limbs; while around the walls are ranged, in almost endless number, skulls, jaws, teeth, and limb-bones of *Toxodon*, *Typotherium*, and *Macrauchenia*, belonging to individuals of all ages and sizes. Perhaps, however, the greatest treasures of this striking gallery are the skull of that remarkable Toxodont which has been described as *Trigodon*, but should properly be known as *Toxodontotherium*, and the skull and jaws of the allied *Zotodon*; the former being from the Monte Hermoso deposits, while the latter comes from distant Catamarca. It must be added that while one of the skeletons of *Toxodon* is formed from the bones of a single individual, the second has been completed from the remains of two animals. And here I may say a word in praise of the admirable manner in which all the skeletons have been mounted and the broken specimens restored by Señor Giacomo Pozzi, the Articulator of the Museum. At the present time the whole of the invaluable series of mounted fossil skeletons are arranged along the middle of the galleries without any kind of protection from injury. Since the palæontological section of the Museum is not yet thrown open to the general public, this unprotected state of the specimens does not lead to much harm, but I may venture to express the hope that when the whole Museum is opened the Government will be fully aware of the priceless value and world-wide interest of these unique specimens, and the necessary steps taken for providing suitable cases for their protection.

Leaving the Toxodont gallery (which is seen in the background on the right side of our third illustration) we pass on to a large hall containing the remains of *Megatherium*. Here we find one entire skeleton of this giant ground-sloth, while facing it is the trunk and pelvis of a second, with the greater part of the fore-limbs attached. Several mounted specimens of limbs and other portions of the skeleton occupy the centre of this chamber; and in the wall-cases are arranged numerous detached bones and some magnificent specimens of the skull, one of the latter being noteworthy on account of its enormous size. In the next gallery we come to a magnificent series of mounted skeletons of the Mylodons and their allies, the more or less nearly complete specimens being six in number (including one of *Scelidotherium*), while there is the greater part of the trunk of a seventh. These mounted specimens range in size from the gigantic *Mylodon*

armatus, which approaches *Megatherium* in bulk, and is characterised by its enormously expanded muzzle and tusk-like anterior teeth, to the comparatively small *Scelidotherium leptocephalum*, which is not very much superior to a tapir in size. In the surrounding wall-cases are displayed an almost endless array of skulls, limb-bones, and vertebræ, many of which are associated. Although all the mounted specimens are from the Pampean formation, some of the wall-cases contain examples of the skulls and bones of *Scelidotherium* from the somewhat older deposits of Monte Hermoso, near Bahia Blanca. These remains indicate species of much smaller dimensions than those from the Pampean, and thus serve to illustrate the general decrease in the bodily size of the members of the various groups of mammals as we descend from the Pampean beds through the Monte Hermoso deposits to the Santa Cruz beds of Patagonia. This decrease is displayed not only among the Mylodonts, but likewise in the Glyptodonts, and the Macrauchenias, as well as in some other Ungulates. For instance, while the Pampean Mylodonts include species equal to the largest rhinoceros in size, the Monte Hermoso *Scelidotherium* was smaller then a tapir, while *Eucholæops* of the Santa Cruz beds was not more than a yard in length, although closely allied to *Mylodon*. Again, among the Glyptodonts, we notice that some at least of the representatives of the genera *Glyptodon* and *Dædicurus* from Monte Hermoso were considerably inferior in size to their Pampean successors, while when we reach the Santa Cruz beds we meet with mere dwarfs, as exemplified by the genus *Propalæhoplophorus*, of which there is a beautifully-preserved skeleton and carapace in the Museum.

Reverting to the Pampean Mylodonts, I may mention that, although the time at my disposal did not admit of my undertaking a detailed survey of any of the Edentates, yet I have little doubt that, if this were done, the number of nominal species in this particular group might be considerably reduced. My opinion has already been expressed elsewhere as to the inadvisability of subdividing the true Mylodonts into separate genera, such as *Lestodon*, *Pseudolestodon*, and *Grypotherium*; while the proposal to split up the group of Ground-Sloths into several families, instead of including the whole in the Megatheriidæ, is not likely to commend itself to English zoologists.

Perhaps the most striking display in the whole Museum is the magnificent series of the remains of Glyptodonts, which are exhibited in one half of the gallery containing the Mylodonts. Here we see not only a fine array of specimens of the carapace and tail-shield, with or without the skull and limbs attached; but likewise a number of entire skeletons without the dermal ossifications. These specimens comprise examples of the genera *Glyptodon*, *Panochthus*, *Dædicurus*, and the one generally denominated *Hoplophorus*; and serve to show conclusively that the original Owenian restoration of *Glyptodon* was incorrect, the terminal tube of the caudal sheath of an *Hoplophorus* having been attached to the carapace of a *Glyptodon*.

May we accordingly venture to suggest to the authorities of the Royal College of Surgeons that the specimen in their Museum thus anomalously restored might now be advantageously dismounted, since in its present state it merely tends to perpetuate an accidental error? Marvellous as are all the Glyptodonts, the most astounding monster in the whole series is undoubtedly the one denominated *Dædicurus*. The total length of this monstrous skeleton, as it is now mounted, is upwards of 11 feet 8 inches measured in a straight line, while the carapace measures 10 feet 4 inches across the highest part of the back, and the length of the massive club-like terminal tube of the caudal sheath is upwards of 3 feet 11 inches. Since, so far as I am aware, there is nothing approaching to a complete skeleton of this strange creature in any European Museum, my readers will probably pardon me if I enter into a few details of its structure. It will be observed, in the first place, that the carapace is remarkable for its curiously hump-backed contour, in which respect it differs very markedly from the regularly egg-shaped shell of *Glyptodon*; while it is further distinguished by the absence of the bold conical bosses with which the periphery of the latter is ornamented. Then, again, there is a marked difference in regard to the structure of the individual plates of which the carapace is composed; for whereas in *Glyptodon* these are polygonal, with a rosette-shaped pattern formed by the impressions of the edges of the overlying horny shields, in the present form they are oblong plates of bone, with a smooth external surface, devoid of the impressions of horny shields, but severally perforated by from one to five large circular holes, through which quill-like bristles were doubtless protruded during life. The tail was protected for the first third of its length by eleven enormous bony hoops, each formed by a single ring of plates similar to those of the carapace, but two of which not unfrequently coalesce, the circumference of these hoops rapidly decreasing from the base of the tail towards its extremity. The terminal two-thirds of the tail are formed by the well-known club-like tube so frequently exhibited in European Museums. At its flattened and expanded extremity, this tremendous club bears a number of roughened, depressed, disc-like facets of an oval contour, which during life must evidently have given support to huge horny spines, probably not unlike the horns of a rhinoceros. The whole animal must accordingly have bristled with horns and quills, looking not unlike some giant porcupine. In the somewhat smaller species from Monte Hermoso, there are more of the disc-like surfaces on the tube of the tail, which also differs from that of the Pampean species by being less expanded at the end and by the presence of a number of flat oval plates on the upper surface. The two imperfect specimens of the carapace of the species of this genus from Monte Hermoso in the collection of the Museum are remarkable for having a crater-like elevation with a central perforation immediately over the point of

attachment of the pelvis. The occurrence of this peculiarity in two examples shows that this feature cannot be an abnormality; but, unless (as Dr. Moreno thinks) it be glandular, I am quite at a loss to guess its rise or object. The skull in this genus has a straight profile from the occiput to the tip of the nasal bones, in consequence of which the oblong aperture of the nose is of great vertical height.

In marked contrast to *Dædicurus* are the plates of the carapace in the allied genus *Panochthus*, which, although oblong in form, have a peculiarly roughened and particular external surface, without perforations for bristles. Occasionally, however, specimens are found showing the impress of horny shields arranged in a rosette-like pattern somewhat after the manner obtaining in *Glyptodon*. The tail of *Panochthus* differed from that of *Dædicurus* in that the terminal tube was less flattened, and not expanded, while it was covered with granules interspersed with disk-like surfaces, which were prominent instead of depressed, and thus evidently bore a different type of spine. More striking is the remarkable difference in the form of the skull, which had a highly vaulted profile, narrow, oblique nostrils, and an enormous descending process to the zygomatic arch. One specimen of the skull has its dermal covering of bone still preserved; each plate consisting of a smooth central disc surrounded by granular bone. Of this gigantic creature, which rivalled *Dædicurus* in bulk, although with a smaller tail, the Museum possesses in addition to several unmounted specimens, one entire carapace, three complete mounted skeletons, exclusive of the carapace, but with the terminal tail-tube, another lacking the tail, as well as a portion of a fifth.

In addition to the peculiarities in the structure of the component bony plates of the carapace already referred to, *Glyptodon* differs from both the genera above-mentioned by the form of the tail-sheath, which is composed of a number of rings, gradually decreasing in diameter, and ornamented with a series of conical knobs, the extremity consisting of a short cone similarly decorated; the similarity of this tail-sheath to that of the extinct Australian tortoise *Miolania* being not a little remarkable. The skull of this genus has neither the straight profile of *Dædicurus* nor the convex one of *Panochthus*, but the frontal and parietal planes meet one another at an obtuse angle, thus causing the nasal aperture to be wider than long. One skull from the Pampean and a second from Monte Hermoso have the bony dermal shield preserved, and show that it was composed of small juxtaposed plates, which become larger and imbricating on the occiput. Of the Pampean forms, which apparently belong to two species, the mounted series in the Museum comprises two skeletons and nearly a score of more or less nearly complete carapaces. There is also a fine series of the remains of the smaller species from Monte Hermoso.

In the genus which we may provisionally allude to under the name of *Hoplophorus*, the skull, as is well shown in a beautiful

example of the entire skeleton, is very different from that of all the allied genera, its chief peculiarity being the curiously-incurved form of the nasal bones, and the consequent involution of the nostrils. The head-shield was long, curved, and smooth, with the largest plates posteriorly. Three specimens of the carapace of this genus are mounted in the gallery; one of these being associated with the head-shield and tail-sheath. The species of this genus from the Monte Hermoso beds appear to have been fully as large as their Pampean representative.

Although I have at present had no opportunity of going into the question of the number of species of Glyptodonts represented in the Pampean formation, I have not the least hesitation in saying that a large proportion of those named on the evidence of specimens of the terminal tail-tube or fragments of the carapace will be found invalid. The same is doubtless true with regard to several of the so-called genera founded on remains from the older formations of southern Argentina. Although it is not my intention to enter here into the consideration of the Glyptodonts of the Santa Cruz beds of Patagonia, I may mention that one of the greatest treasures of the Museum is the entire skeleton and greater portion of the carapace of the dwarf *Propalæhoplophorus*, to which incidental allusion has already been made. As shown both by the conformation of the skull and tail-sheath, these dwarf Glyptodonts were much more nearly allied to *Glyptodon* than to either of the other Pampean genera, all the latter being probably, therefore, more specialised types of later origin.

Although in the Pampean formation the short-snouted Glyptodonts appear to have been the dominant types of the Loricate Edentates, it must not be inferred that the long-snouted Armadillos were absent. While some of these forms found in the deposits in question were more or less closely allied to, or even identical with, their living cousins, others, like *Eutatus*, were of much larger size, and differed by having the whole carapace formed of movable bands. Still larger was a recently-discovered armadillo, for which the name *Dasypotherium* has been proposed; the skull of this giant measuring upwards of 10½ inches in length. Remains of the existing genus *Dasypus* are not uncommon in the Pampean, and the Museum possesses some beautiful examples from Monte Hermoso, one of which shows both the skull and carapace. I may mention here that a species of *Dasypus* allied to the existing *D. minimus* (which it has been recently proposed to separate generically as *Zaëdius*) occur in the Santa Cruz beds; a fact which has an important bearing on the geological age of the latter.

Before leaving the mammals of the Pampean beds, I must not omit to mention the fine series of equine remains contained in the Museum; although in regard to one or two species, the La Plata collection is inferior to that of the Buenos Aires Museum. Among the more notable specimens, I may refer to a skeleton of an *Equus*, which has been assigned by Señor Ameghino to a so-called species

which he terms *E. rectidens*, but which, like the other specimens so-named, I see no reason for separating from *E. curvidens* of Owen. Although possessing no complete skulls, like those in the Buenos Aires collection, the La Plata Museum also contains a fine series of the remains of those horses which have been generically separated under the name of *Hippidium*. Hitherto I have not considered this separation justifiable, but from the study of the actual specimens, I am now convinced that it will be convenient to regard the extreme backward elongation of the nasal slits characterising these extinct horses as a feature of generic value. And here I may mention an instance of that want of appreciation of differences due solely to individual peculiarities and variation in age which unfortunately characterises so much of the palæontological work of Ameghino. On the evidence of a single lower equine molar from the Parana, that gentleman proposed to establish a genus stated to differ from other Equidæ by the almost total absence of folds of enamel in the cheek-teeth, for which the name of *Hipphaplus* was suggested. As a matter of fact, this tooth is nothing more than an extremely worn molar of *Hippidium*, as is conclusively proved by a large series of Pampean specimens in the Museum, which exhibit a complete transition from the unused to the much-worn type. To make matters worse, when the error was pointed out by Burmeister, the founder of the so-called genus deliberately set to work to justify his own views, instead of frankly acknowledging his error. The existence of a genus closely allied to *Equus* in the Parana beds, which are set down as of Lower Oligocene age, would not, however, by any means have suited the views of the Argentine palæontologist, and hence *Hipphaplus* must be maintained at all hazards!

The third genus of South American equines is represented by a very remarkable skull recently discovered in the Pampean deposits on the coast of the province of Buenos Aires, and first described by Dr. Moreno under the name of *Onohippidium*. While agreeing with the skull of *Hippidium* in the extreme elongation of the nasal slits, this specimen is distinguished by the presence of an enormous lachrymal fossa of an oblong form and of great depth. Although it is certain this fossa must have contained a large lachrymal gland, its size is much greater than in any other mammal, either living or extinct, with which I am acquainted. The cheek-teeth were of the general type of those of *Hippidium*.

(*To be continued.*)

R. LYDEKKER.

IV.

Note on the Air-Sacs and Hollow-Bones of Birds.

MR. F. W. HEADLEY, in his interesting paper in the November number of NATURAL SCIENCE (vol. iii., pp. 346-356), calls attention to the fact that while the Gannet and the Cormorant belong to the same order, the one has large air-sacs and a remarkably pneumatic skeleton, while the other is devoid of air-sacs and has practically non-pneumatic bones. I have said practically non-pneumatic, because in *Graculus carbo* and related forms, the humerus and sternum have small pneumatic foramina, while in the extinct *Phalacrocorax perspicillatus* and other grooved-billed species these bones are quite non-pneumatic. Mr. Headley suggests that light bones would be out of place in, or unserviceable to, diving birds, and this is undeniably the case, since the problem with them is how to keep under water.

While the solid body and dense bones of the Cormorant are directly correlated with its mode of life, the air-sacs and pneumatic skeleton of the Gannet appear to be equally good adaptive characters. The Cormorant pursues its prey beneath the water, the Gannet plunges upon its victims from above, dropping upon them headlong from heights of one to two hundred feet. Now, as the cunning mechanic has devised an air-chamber to break the accidental fall of an elevator (American for lift), so nature has applied about the neck and breast of the Gannet a series of air cushions to break the shock which occurs when a bird weighing six or eight pounds strikes the water after a drop of a hundred feet or so.

In the Brown Pelican (*Pelecanus furcus*) there are many air-cells present about the breast forming a thick mass of loose areolar tissue located exactly where they would do the most good as a buffer when this big bird plumps (dashes is hardly the word to use in connection with a pelican) down upon a shoal of fish. I have never dissected any of the white pelicans, and as these birds fish in quite a different manner from their darker relatives, it would be interesting to know if there is any corresponding difference in the arrangement or number of the air-sacs.

Another point possibly served by the air-sacs, as well as by the pneumaticity of the bones, is that of equalising the pressure of the air when a bird either dives from air into water, as does the Gannet,

or rapidly descends from a great elevation in the air, as do some of the vultures. Soaring birds, such as the vultures, screamers, and some storks, certainly have very pneumatic bones; but I do not pretend that the above theory accounts for the fact, it is merely a suggestion as to one use of the character, and while the theory works very well when applied to such birds as the Gannet and Vulture, it fails in the case of the hornbills. Here, perhaps, we may fall back on the very convenient and much-abused theory of an inheritance from some active ancestor, but this again is merely a suggestion.

It is somewhat curious that of two nearly related birds one may have certain bones pneumatic, and the other non-pneumatic, a point well illustrated by the titmice. It is, too, a general rule that animals of rapid movements have light bones, and sluggish or aquatic animals non-pneumatic skeletons; and aside from the gain in size without increase of weight, which is rendered possible by a hollow bone, may there not be an actual oxygenation of the blood in the bones?

FREDERIC A. LUCAS.

V.

Cell-division.

ALTHOUGH the main general features of cell-division are familiar to most readers of NATURAL SCIENCE, yet it is probable that not a few have been unable to find time to keep pace with even a part of the ever-increasing literature on the subject. So many varied, and in some cases contradicting, statements have been made recently, as to the real nature of the processes of cell-division, that any attempt to summarise them as probable truths is necessarily accompanied with considerable difficulty, and a likelihood to prove unintelligible or misleading. Nevertheless, it is hoped in this article to give some account of the chief features of ordinary cell-division looked at in the light of the latest researches. Ever since Strasburger, Bütschli, and Fol in 1870 drew attention to the extraordinary changes which generally take place in the nucleus of a dividing cell, a large army of investigators have given their almost undivided attention to increasing our knowledge of the subject, and it is a matter of no small regret that in such a wide field of research, which affords so many chances of gaining deeper insight into some of the most striking phenomena exhibited by living matter, few, if any, English names stand pre-eminent.

A cell may divide either (*a*) mitotically, that is to say, may pass through a process of change known as mitosis or karyokinesis, whereby a very perfect division of the chromatin, *i.e.*, the deeply-staining portion, in the nucleus, is brought about, or (*b*) amitotically, where a rough-and-ready splitting of the nucleus is substituted for the more elaborate mitotic division. The former is by far the more usual of the two, and although there are undoubted cases where amitotic division is the rule, it is probable that karyokinesis is even of wider occurrence than it is thought by many to be. It is with this form of nuclear division that the present contribution exclusively deals, and it will be found convenient to study the subject from the following points of view, viz.:—

(1.) The general features of karyokinesis.

(2.) The origin, nature, and function of the component parts of the nuclear figure.

(3.) The relation of karyokinesis to fertilisation and segmentation.

(4.) Theoretical considerations.[1]

The General Features of Karyokinesis.

In Figs. 1-8 (p. 43) the nucleus of a cell is represented in the various phases through which it passes during karyokinesis. In Fig. 1 the nucleus is in the resting condition, the chromatin being in the form of a much-coiled thread. The first preliminary stage towards division is marked by the thread acquiring a sharper contour, due to the withdrawal of numerous small branches and processes. It then proceeds to break up into a number (eight in this case) of pieces, Fig. 2. Each of these splits longitudinally into two exactly similar halves, forming in all sixteen daughter chromosomes (*Chr.*), as the masses of chromatin are now called, Fig. 3. The nuclear membrane at this period becomes indistinct and gradually disappears, and a new body comes into play, viz., the centrosome. Whatever may be its origin, the centrosome, owing to the active part it takes in the next stages, must be noticed at this point, though it will be dealt with in detail later on. A centrosome is thought by many to be an integral part of, and therefore present in, every cell during the resting as well as the active state. Since, however, it is unaffected by ordinary staining reagents, its presence is often difficult to demonstrate. At the moment when the nuclear membrane begins to disappear, the centrosome becomes surrounded by a number of radiating strands of protoplasm (Fig. 4, *c.*), which form the so-called "archoplasmic sphere." The centrosome then divides into two, each half remaining attached by means of numerous threads (*C. Sp.*). These threads are, in this case at any rate, the beginning of the future nuclear spindle, although in other cases a different origin has been assigned to it, as will be seen when the spindle is dealt with by itself. The next stage is represented in Fig. 5. The centrosomes have greatly increased their distance from one another, owing to the rapid enlargement of the spindle. The chromosomes (*x.*) have grouped themselves round the spindle in the median plane, to form the so-called equatorial plate. They have, furthermore, become doubled, each pair on itself, in such a way that the angle of the bend is directed towards the spindle, while the two ends point towards the surface of the cell. The next two stages are shown in Figs. 6 and 7. The two daughter chromosomes of each original chromosome gradually recede from one another and move along the spindle, until they come to lie, one set at one pole of the spindle and the other at the other. Nevertheless, they are not as yet entirely separated from one another, but still remain connected by delicate threads (Verbindungs Fasern, Hertwig) formed from the so-called "lâme intermédiaire" of Van Beneden. These must not be confounded with the threads comprising the spindle, from which they are quite distinct.

[1] It has been found possible in this article to treat of only the first two of these headings.

The condition shown in Fig. 7 is called the dyaster. In Figs. 5–8 only half the number of original chromosomes have been drawn. The last phase (Fig. 9) is marked by the disappearance of spindle, centrosomes, archoplasmic spheres, etc., and a return to the resting condition, accompanied by division of the body of the cell. At the moment when, in the dyaster stage, the archoplasmic spheres have attained their maximum development, the division of the cell-body commences. This usually takes place by a nipping of the cell transversely to the axis of the spindle. The furrow grows deeper and deeper inwards, until it cuts the cell into two nearly equal halves. As soon as this is accomplished, the archoplasmic spheres begin to disappear, and the chromosomes rapidly pass into the resting condition, apparently by imbibition of fluid. This stage (Fig. 10) has been termed the dispirem The chromosomes eventually break up into small grains, and a vesicular nucleus is formed, with the chromatin substance distributed as a network or coiled thread. This passage of the chromosomes from an active to a passive condition has been followed out most carefully by Boveri, who has shown that they preserve their individuality throughout the whole of the resting stage. The nuclear membrane is apparently formed by a concrescence of some of the small grains of chromatin into which the chromosomes have broken down.

The above facts may be looked on as being typical of all forms of karyokinesis, as far as regards the behaviour of the chromosomes; in the cells of different animals, however, these may vary considerably in number and appearance, from a few large threads, as in the above instance, to a large number of more or less irregular granules. Furthermore, although an exact splitting of each chromosome of the nucleus of the parent cell has not been definitely observed in all cases, especially where the chromosomes are small and numerous, it may be almost certainly assumed that some sort of process of this kind invariably does take place, either during the resting stage or when the cell is undergoing karyokinetic change. Before going on, however, to more debatable ground, where not only the interpretation of the facts, but even information as to the facts themselves, varies as we study the works of one author or of another, certain abnormalities observed in karyokinesis deserve a passing mention. It has been found that cell-division can be influenced in a remarkable way by varying the external conditions under which it is placed. The brothers Hertwig have given an account of some experiments performed on the eggs of the sea-urchin, *Strongylocentrus*, of which the following are the most remarkable.

Intense cold, *e.g.*, 2° C., has been shown to destroy all parts of the nuclear figure excepting the chromosomes. Dilute quinine appears to have an extraordinary effect on the centrosome, as a result of which it divides into four or five, each surrounded by its archoplasmic sphere, and consequently the figure may have four or five polar stars

instead of two. Perhaps, however, the most curious of all these pathological cases are those of ova which have been in some way enfeebled, and so are unable to prevent the entrance of more than one spermatozoon, a phenomenon which in these forms is abnormal. Many of these spermatozoa come into contact with the egg nucleus. There results a karyokinetic figure having three, four, or many spindles and consequently double as many polar stars as there were spermatozoa which reached the egg nucleus. Furthermore, those spermatozoa which have entered the ovum but not come into contact with the egg nucleus, may also divide karyokinetically, and combine together to form figures and patterns of most varied kinds.

The Component Parts of the Nuclear Figure.

It is now necessary to consider more in detail the various elements which go to make up the nuclear figure. From the foregoing account it will be seen that the parts of a karyokinetic figure can be tabulated in the following way:—

Nuclear figure	Achromatic elements.	Centrosomes. Archoplasmic spheres. Spindle. Lâme intermédiaire.
	Chromatic ,,	Chromosomes.

Centrosomes and Archoplasmic Spheres.—Although the so-called "Polkörperchen," or "centrosome," was discovered as early as 1870, by Fol and others, to exist in the centre of the archoplasmic spheres during the division of the nucleus, yet even at the present time opinions vary considerably as to its real origin. Van Beneden, Boveri, and indeed the majority of observers, look upon the centrosome as a permanent organ of the cell-body, present during the resting as well as active stage, and entirely external to the nucleus. Their views are to some extent borne out by the fact that in several instances, *e.g.*, epithelial cells of fishes and cœlomic corpuscles of Nemertines, an undoubted centrosome has been found during the resting stage in close proximity to, but perfectly distinct from, the nucleus. On the other hand, in few, if any cases, has the centrosome been actually traced through the stages of karyokinesis into the resting condition. O. Hertwig, on the other hand, is opposed to this view, and maintains that the centrosome is in every case essentially a nuclear structure, for the following reasons: (*a*) it has not been demonstrated, save in a few exceptional cases, as existing in the body of the cell during the resting stage; (*b*) its appearance is usually simultaneous with the breaking up of the nuclear membrane; (*c*) it has been observed to appear first at the edge of the nucleus, and then gradually wander into the body of the cell.

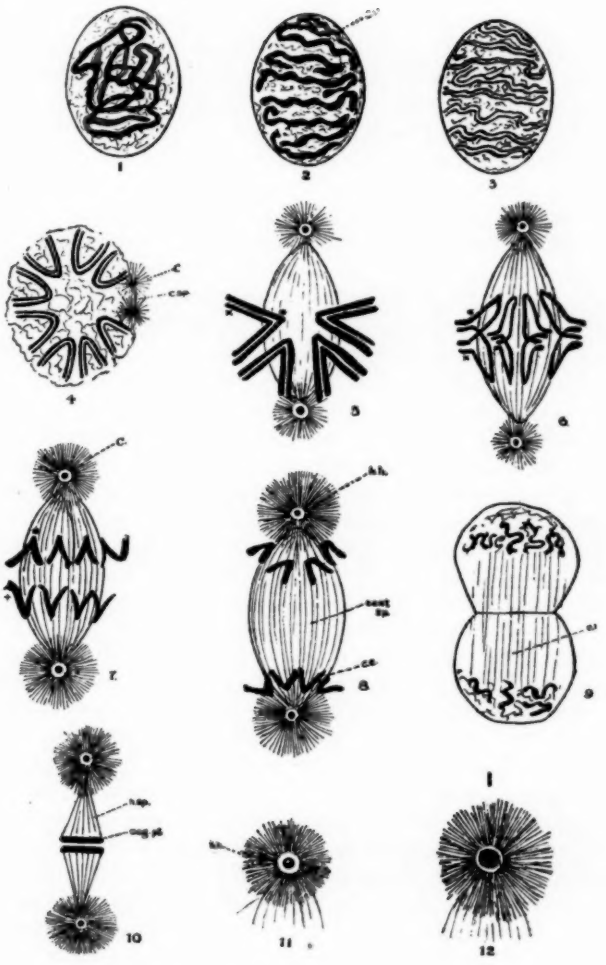
Although the opinion of Hertwig is contrary to that of most authorities, yet Brauer in Germany, and Moore in this country, have recently (September, 1893) published accounts of spermatogenesis, the former in *Ascaris megalocephala*, the latter in *Branchipus*, in

which they have apparently conclusively demonstrated the nuclear origin of the centrosome.

Mr. Moore has been able to observe the actual formation of two distinct centrosomes, by fusion of bodies he calls dictyosomes, which apparently are parts of the achromatic reticular network. If Mr. Moore's conclusions can be applied to other cases, many previous existing ideas as to the nature of the centrosome will have necessarily to undergo considerable modification. Meanwhile, his results must be looked upon as forming an isolated exception. Brauer, on the other hand, considers the centrosome to be always present in the nucleus as a permanent body, which may or may not pass through the nuclear membrane preparatory to setting up karyokinetic division. As will be seen later on, in discussing the possible phylogenetic history of the centrosome, it is probable that the views of Hertwig on the one side, and Boveri on the other, may be equally true for different cases.

Peculiar interest was aroused when it was shown that the "Mittelstück," *i.e.*, that part which separates the head and tail of the spermatozoon, was really a centrosome, taken by the spermatozoon into the egg as an organ of division, a fact supported by all the more careful researches. Furthermore, Fol has shown in the case of the egg of an echinoderm, that the sperm-centrosome (*i.e.*, the "Mittelstück") and egg-centrosome take up their position one at either end of the first segmentation nucleus in the region of the future equatorial plane. Each then divides into two, and the halves travel in opposite directions, thus resulting in four centrosomes, one lying at each corner of the somewhat square-shaped nucleus. Each half of the original sperm-centrosome fuses with half of the original egg-centrosome, thus forming two centrosomes, both of which are partly paternal and partly maternal. This done, segmentation commences in the ordinary way. Although the above process takes place in one case at any rate, it is by no means so certain that it occurs in the large majority of instances which have been studied. On the contrary, it has been found that the centrosome of the egg almost invariably degenerates and disappears before the sperm-nucleus comes into contact with that of the ovum, and consequently every centrosome in every cell of the body of the resulting organism is derived from "Mittelstück" of the spermatozoon. The view that this egg-centrosome is typically a rudimentary organ was first proposed by Boveri, and his conclusions have been verified by Vejdowsky for *Rhyncelmis*, and Fick in his admirable paper on the maturation and fertilisation of the egg of *Axolotl* (*Ann. Mikr. Anat.*, Nov., 1893).

As to the explanation of these facts, it would seem probable that the degeneration of the egg-centrosome takes place in order to prevent parthenogenetic segmentation of the ovum. This supposition may appear absurd to those who look upon the egg nucleus after the giving



FIGURES OF THE CELL-NUCLEUS DURING KARYOKINESIS.

off of the polar bodies as thereby incapable of further division without additional chromatin elements derived from the spermatozoon. Boveri, however, has shown that, in certain cases, the spermatozoon may remain in the periphery of the ovum instead of travelling towards the egg nucleus. The sperm-centrosome, however, does not likewise remain inactive, but approaches the egg nucleus and divides into two. The egg nucleus then divides karyokinetically, and with it the whole body of the ovum. The spermatozoon only comes into activity when the 4- or 8-celled stage has been reached, when it fuses with the nucleus of that blastomere in which it happens to lie. In this case it is obvious that the egg nucleus, even after giving off the polar bodies, has sufficient chromatin left to enable it to divide and reproduce the organism, but at first lacks the organ whereby that division can be brought about. Many facts of this description could be adduced to prove that the presence of a centrosome is necessary for division to take place, and since it has been shown that a spermatozoon can "fertilise" a non-nucleate fragment of ova, and develop into larvæ of exactly the same species as the animal from which the spermatozoon was taken, the following important conclusion must be drawn as applicable to most cases :—A mature ovum and a mature spermatozoon have both sufficient nuclear matter for the production of a new individual, but cannot do so independently—the ovum because it lacks a centrosome, the spermatozoon because it has not the requisite quantity of protoplasm. The object of fertilisation is therefore that each may supply the wants of the other—the fertilised ovum possessing both a centrosome and a sufficient quantity of protoplasm. The advantage of additional chromatin elements derived from the spermatozoa will be dealt with later.

As to the nature of the archoplasmic sphere little need be said. It appears to be composed of radiating strands of protoplasm, which, however, do not arise from the centrosome. They do not probably come at all into contact with it, being separated from it by the so-called "heller hof." How they are formed and what is their meaning is at present very little understood.

Turning now to a consideration of the probable phylogenetic history of the centrosome, the presence of a macro- and micro-nucleus in many Ciliata at once suggests possible homologies. It is unnecessary to go into the details of the nuclear changes which take place in conjugating Infusoria, so admirably worked out by Maupas, for it will be remembered that the micro-nucleus only is the active and important structure corresponding to the nucleus in the cells of the metazoa, the macro-nucleus merely degenerating, to be formed afresh by division of the reconstructed micro-nucleus. Can the centrosome be homologised with either of these? This, of course, is very doubtful; but if any homology is to be drawn, it would appear probable that the centrosome is more nearly related to the macro-nucleus. This may seem improbable, since it is the view of most

authorities that if any comparison can be made it is between the centrosome and the micro-nucleus. Some recent researches, however, on the ova of vertebrates and arthropods have revealed the fact that the centrosome of the ovum, although functionless, is relatively of enormous size, and remains for a considerable time, even until the embryo has reached an advanced stage in development. Later on it disappears. If the macro-nucleus of a ciliate protozoon be looked upon as a centrosome which, like the one just described, has lost its power of setting up nuclear division, and is merely a rudimentary organ, it can be homologised, or, at any rate, closely compared with the centrosome of an ordinary ovum, which, as a rule, degenerates and disappears. Hence, whether the centrosome be considered as a nuclear organ or as belonging solely to the cell-body, it matters little; for it is easy to imagine the former case to be the more primitive condition, and, from evidence gathered from different sources, we are almost compelled to allow that both conditions actually do occur. The following table may, therefore, be drawn up as representing the probable phylogenetic history of the centrosome.

(1.) A most primitive condition, where one nucleus only is present, which seems to possess not only the hereditary chromatin matter, but also power of spontaneous division, *e.g.*, *Amœba*.

(2.) A condition where the primitive nucleus becomes differentiated into two; one, the micro-nucleus, possessing the hereditary substance, the other, corresponding to the macro-nucleus, which functioned as an organ of division. Probably the two nuclei were of more equal size than are the macro- and micro-nuclei. Unrepresented as far as is yet known.

(3). The macro-nucleus has dwindled to become a centrosome. *E.g.*, Spermatozoa, tissue cells, and ova where the centrosome is practically active.

(4.) The centrosome is rudimentary and disappears, to be replaced by that brought in by the spermatozoon; thus the primitive condition of one nucleus is for a short time regained between the giving off of the polar bodies and fertilisation. This nucleus, however, has not the power of spontaneous condition. *E.g.*, inert ova.

If these are the probable stages in the phylogenetic history of the centrosome, it will be seen that the condition obtaining in such forms as *Paramœcium* is an offshoot of No. 2. The macro-nucleus, after losing its function as a centrosome, has probably increased largely in size and become a storage nucleus. The micro-nucleus has taken on double duty, so to speak, *i.e.*, it is not only the bearer of hereditary substance, but is also capable of spontaneous division.

It is by no means illegal to make this supposition of a change of function for the macro- and micro-nuclei respectively, since such a phenomenon is of constant occurrence in the animal kingdom. In the afore-mentioned cases of vertebrate and arthropod ova, a most primitive condition seems to exist. Although the macro-nucleus or

centrosome has lost its function, it retains its original size and shape, and the fact that it surrounds itself with an archoplasmic sphere, points to some of its original power being still left. Furthermore, there can be little doubt that, whether we homologise the centrosome with the micro- or macro-nucleus, it was originally derived from the single primitive nucleus. Where it appears to come out of the nucleus before, and to go back again after, karyokinetic division, it is merely a recapitulation of its past history. A point in advance has been gained by those cells where the centrosome remains permanently outside the nucleus.

No mention has been hitherto made of the remarkable observations of Herr Lauterborn on living diatoms, an account of which appears elsewhere in this number of NATURAL SCIENCE (p. 7). His results are so at variance with all previous known facts of karyokinetic division, that at present it seems safer to draw no conclusions until more confirmatory evidence has been obtained. Besides, the mechanics of the processes which he describes are completely unintelligible.

The spindle.—What has been said with regard to the nuclear or non-nuclear nature of the centrosome applies equally well to the spindle. It appears to be formed by the activity of the centrosomes. Where the latter remain permanently in the cytoplasm, the spindle would appear to be made up of strands of protoplasm resembling those which make up the archoplasmic spheres appearing between the two daughter centrosomes. Where the centrosome has an intranuclear origin, the spindle is undoubtedly formed from the achromatic portion of the nucleus.

Furthermore, two kinds of spindle must be distinguished, viz.:—(1) The so-called "central" spindle; (2) the spindle which is made up of two "half-spindles." An instance where the former obtains is that taken as a typical case of karyokinesis. The second kind of spindle is shown in Fig. 9. It will be seen that it is discontinuous in the equatorial plane, the fibres merely extending from the archoplasmic rings to the chromosomes. The function also of the two seems to be different. The two half-spindles appear to exert a pulling on the chromosomes, whereas the central spindle is probably a groundwork in which the chromosomes move by their own activity. Van Beneden derives the spindle from both the cytoplasm and the achromatic network of the nucleus.

It would, however, seem probable that, where the spindle is derived from the protoplasm of the cell (as it appears to be so in those cases where the centrosomes are permanently extra-nuclear), it takes the form of two half-spindles; but where it is formed from the achromatic network in the nucleus, it is of the "central" type. This point has not yet received sufficient attention. It may, moreover, be urged against the assumption of two different kinds of spindle, that the supposed existence of the central spindle is due to a wrong con-

ception of the "lâme intermédiaire" as being part of the spindle. On the other hand, it is possible that the two half-spindles are merely the rays of the archoplasmic spheres, and in no way to be compared to the central spindle. These points obviously still require considerably more light to be thrown upon them.

The lâme intermédiaire.—This name was given by Van Beneden to the layer of achromatic substance between the split halves of the chromosomes. It consists chiefly of delicate fibrils (Verbindungs Fasern) which lengthen out as the chromosomes increase the distance from one another. Brauer has attempted to distinguish between these fibres and those that form the central spindle, on the ground that the latter are actively contractile while the former are merely connecting threads, but whether this is so or not, there can be little doubt that these connecting threads are formed from some portion of the achromatic network of the nucleus, even in those cases where the spindle may have an extra-nuclear origin. In some cases they appear to persist longest of any of the elements of the karyokinetic figure, remaining visible even if the daughter cells have become completely cut off from one another. The chromatic elements of the nuclear figure will be dealt with in a future paper, but there are two points which should here be noticed, viz.: (*a*) The mechanics of karyokinesis. (*b*) The fate of the nucleolus.

(*a*) *Mechanics of karyokinesis.*—More, perhaps might be written on this part of the question than on any other, but it would be almost entirely of a speculative nature. Nothing is really known as to how the chromosomes move towards the poles, although it is the opinion of many that the threads of the spindle are contractile, and so pull the chromosomes apart. Exactly the contrary is the view of Strasburger for plants, and Watase for the cells of embryo Cephalopods. The former believes that the threads of the archoplasmic spheres penetrate through the porous nuclear membrane, forming two half-spindles. The movement of the chromosomes, he considers, are due partly to their own independent activity, and partly to the fact that they are pushed towards the poles by swelling up of the "lâme intermédiaire" due to the osmosis of nuclear fluid.

Watase, on the contrary, although he believes with Strasburger that there is no central spindle, but only two half-spindles, considers that the threads are stiff, like bristles, and flatten out the nucleus into an equatorial plate. By this means they gradually push the chromosomes between one another, eventually carrying one set to one pole and one to another, the membrane of the nucleus being preserved during the whole process. This view, which Watase thinks to be of general application, seems somewhat fantastic; yet, at the same time, it must be allowed that his figures strongly suggest such a process. It has recently been suggested that the centrosome is by no means a

definite structure, but is merely an expression of the peculiar substance in the archoplasm, and has attractive power on the chromosomes. Ed. van Beneden considered that the spindle threads were contractile, and fixed into the centrosome as a fulcrum. More careful examination has shown that the centrosome is invariably separated from the striated archoplasmic sphere by a light area ("heller Hof" of Boveri), in which there are no fibres or striations. It is, therefore, best to dismiss the idea of contractile threads, and to consider that the chromosomes move by virtue of their own activity along the threads of the spindle. That they do so under some sort of chemiotactic influence derived from the centrosome is also very probable, nay, almost certain. Fick has, in his paper on the fertilisation of the egg of the axolotl, shown that the spermatozoon, after entering the ovum, turns completely round, so as to bring the "Mittelstück" foremost in the direction of the centre of the ovum. The latter proceeds towards the egg nucleus, while the former follows a short distance behind by means of amœboid movements. If in this case a centrosome can apparently exercise such a strong attractive power on the sperm nucleus, which is nothing more than a mass of chromatin, why should it not have a similar influence on the chromosomes in a nuclear division?

As opposed to this view it may, however, be asked, What is the use of the spindle if the centrosome can act so powerfully on the chromosomes? In answer to this, it is possible to imagine that, owing to the necessity for perfect halving of the chromosomes, a definite path is necessary for each chromosome to move along in order to prevent confusion. Haecker believes that he has discovered the actual product which acts chemiotactically on the chromosomes. Figs. 11 and 12 show this. Fig. 11 represents a centrosome surrounded by its archoplasmic sphere at the time when the equatorial plate is just being formed. It will be noticed that the "heller Hof" (*hh.*) is large, and that the centrosome has somewhat of a vesicular appearance. Fig. 12 is drawn at the moment when the chromosomes have begun to migrate towards the poles. It will be seen that the centrosome now appears as a homogeneous granule, while between it and the "heller Hof" is a zone of a staining semi-fluid consistence. This Haecker believes to be the contents of the centrosome, which are thrown out, and exert the attracting influence on the chromosomes.

Haecker's work was chiefly carried out on Cladoceran eggs. His views must be considered as yet to be purely speculatory, especially as they do not appear able to fit in with facts described by other observers.

(*b*) *The fate of the Nucleolus.*—The word "nucleolus" has, unfortunately, to do duty for at least three distinct components of the nucleus, viz.:—(1.) The true nucleoli, which appear to be part of the achromatic network, and are unaffected by ordinary staining fluids.

It has been suggested that the centrosome is formed by a concurrence of two or more of these nucleoli. (2.) Lumps of chromatin which may be numerous, few, or even represented by a single mass. The fate of these structures is quite easy to follow out. They break down to form the chromosomes. (3). The so-called "germinal spots" which exist in the ova of many animals. They may be either masses of deeply-staining nuclear substance, with or without vacuoles; there may be one large germinal spot consisting of two distinct parts, a smaller deeply-staining moiety, and a large, more vesicular portion. The smaller of these nucleoli appear to run together to form one larger one, in which the vacuoles are rhythmically contracting. Thus, in some cases at least, the germinal spot probably acts as an excretory organ to get rid of the waste products of the actively-growing chromosomes. The vacuoles run together into one large one, which then bursts. The whole process has been watched in living eggs, and occupies from four to eight hours for each systole and diastole. What the actual fate of this kind of nucleolus is, is not yet certain, although it has been observed to maintain its individuality for some time after the dissolution of the nuclear membrane. It then suddenly disappears, and is, on this account, possibly to be looked upon as chromatin which has taken on a special function, and consequently become useless as hereditary substance. Hence its apparent dissolution when the nucleus passes into the active condition.

These, then, are the main facts of Karyokinesis. It is hoped, in a future article,[2] to discuss its important relations to those theories of Heredity which locate the hereditary tendencies in the chromosomes of the nucleus, and also to compare Mitosis with Amitosis, which latter, in all probability, was the most primitive form of cell-division.

M. D. HILL.

[2] With that article will be given references to the more important papers bearing on the whole subject.

VI.

Recent Researches on Olive-Brown Seaweeds.

ALTHOUGH the nature and functions of the contents of the conceptacles of the Fucaceæ were well-known previously, there was no definite account of the development of these bodies themselves until 1880, when the first thorough examination of the subject was made by Professor Bower (1). He gives the views of earlier writers, and tells us that Reinke looked on the bodies variously called Fasergrübchen, cryptostomata, vegetative and neutral conceptacles (simply conceptacles full of hairs but without antheridia or oogonia) as the "type of these structures" originating "by a separation of four or five neighbouring cells of the limiting tissue from one another. He compares this process with the formation of the resin-passages in the Coniferæ."

It is interesting to see that Luerssen published four years later a different view, and one nearer the truth, though he does not describe the development of the conceptacle in any detail.

Professor Bower next made an investigation of the subject, and described the whole development of the Fucaceous conceptacle in all its detail. He showed that "the division of the outer cell of the limiting tissue of the thallus by vertical walls into four, ceases in certain cases. The division by walls parallel to the surface, however, continues. A linear series of cells is thus formed which may be traced some distance into the tissues, but which is terminated by a single cell only. Later, the activity of division in the horizontal direction also ceases, and as the terminal cell of the series does not increase in size, the result is that it is surpassed by the tissues surrounding it. The terminal cell of this series we may call the 'initial' cell of the conceptacle, the cell immediately beneath it may be termed the 'basal' cell."

The development of both cryptostoma and conceptacle is shown by Professor Bower to be the same, and this fact has, of course, given rise to theories as to which was the earlier of the two in the history of the Fucaceæ. Professor Bower takes the view that the cryptostoma is an abortive conceptacle, and proposes the term "neutral" or "sterile" conceptacle. Oltmanns (2), on the other hand, takes the view that the cryptostomata are the older of the two, and have in course of time come to bear oogonia and antheridia; while I (3)

have ventured, in describing the anatomy of the genus *Turbinaria*, to take a third view, viz., that both conceptacles and cryptostomata are of equal antiquity, and that neither is an outcome of the other.

For some reason that I hardly understand, all these views, and apparently mine in particular, have supplied a source of innocent merriment to Mr. George Murray, who, both in the *Journal of Botany* and his *Phycological Memoirs*, has treated the "ancestors of the Fucaceæ" as if they came from Wardour Street. As will be seen later on, fate has been so ironical that his researches have thrown fresh light on the whole dark subject and possibly a way out.

The matter was in this stage when, as Mr. Murray has since shown, an important side-light was thrown on the subject by the investigation of the development of the conceptacles of *Splachnidium*. This strange-looking plant had been often mentioned and described, but never properly investigated, till Mr. Murray, suspecting that it might prove interesting in connection with the question of cryptostomata, handed it over to Miss Mitchell and Miss Whitting for examination. The result (4) exceeded his anticipations. The first point of interest was the strange persistent apical cell which the authors describe in the following way :—

"The apical cell in *Splachnidium rugosum* differs entirely in appearance from the surrounding meristem; it has a pear-shaped body bounded by a thick transparent mucilaginous wall, and a filiform basal process stretches towards the interior of the thallus. This cell and the very active surrounding meristem are sunk in a slight depression; the two outer layers of the meristem by radial division form the epidermal cells, which continue to divide radially until the part of the thallus in which they lie has reached maturity."

. . . . . .

"The apical cell persists throughout the life of the plant, and not only does it appear at the apex of the main branch, but cells of the same kind are found at other points where growth is taking place, *e.g.*, in the young conceptacles and at the point of formation of lateral branches. It seems as if its appearance were the herald of every local development."

Except for a slight difference in the casting of external layers of the mucilaginous cell-wall, and in the junction of the process at its base with the filaments of the thallus, the initial cell of the conceptacle was found to resemble in every particular the apical cell described above. Former investigators had supposed these conceptacles to be cryptostomata, like those of Fucaceæ, to which this alga was supposed to belong, and in their earlier stages they are exactly similar, "the tufts of hairs extending," like those of *Cystoseira*, "a long distance beyond the ostioles." Mr. Murray (6), in comparing these points in *Splachnidium* and *Fucus*, says: "In *Fucus* it will be remembered that hairs are not produced until the formation of a young conceptacle with an ostiole, while the initial cell has decayed. The

hairs in the conceptacle of *Splachnidium* are long, septate, and unbranched, increasing in length by successive divisions at the base, and giving the young conceptacle the appearance of the cryptostomata of the Fucaceæ. The sporangia are developed later. We have, in fact, in this type a sorus of sporangia and paraphyses comparable with those of *Laminaria*, rolled up and definitely limited in a conceptacular body, produced, not exactly in the same manner as the Fucaceous conceptacle, but after another manner directly comparable with it."

The occurrence of cryptostomata is by no means confined to the Fucaceæ; we find them in genera of Laminarieæ—*Alaria*, *Saccorhiza*, and *Adenocystis*. They also occur on other algæ, such as *Hydroclathrus* (Scytosiphonaceæ), described by Miss Mitchell (5), and on *Chnoospora* (Sporochnaceæ).

As regards the Laminarian cryptostomata, Mr. Murray, who has examined them himself, writes: "I have been so fortunate as to obtain good examples of their occurrence in *Saccorhiza bulbosa* and *Alaria esculenta*. *Alaria* has been frequently described as possessing cryptostomata. Greville definitely refers to them as "minute pores from which issue minute tufts of filaments"; and Professor Bower refers to them also in passing. I have not placed myself in antagonism to these authorities on this minute point, without arming myself with certainty . . . . No pits or conceptacles are formed, and in a very old *Alaria* frond which I have examined, there was at most a depression one cell deep. The *Alaria* cryptostomata, if I may call them so, are tufts of hairs of basal growth, the cell at the apex of the hair being first cut off from the epidermal layer. On Plate XVI., Fig. 7, I have illustrated the mature cryptostoma of *Saccorhiza bulbosa*. So far as I am aware, the mature stage of these bodies in *Saccorhiza* has not been figured before, and the reader who compares this figure with that of the young conceptacle of *Splachnidium*, while it yet bears hairs only, cannot fail to be struck by the resemblance in all respects except the persistent initial cell of *Splachnidium*. We have here, then, a body which has every claim to rank as a cryptostoma." Mr. Murray then alludes to Mr. Setchell's paper on *Saccorhiza dermatodea*, in which he has figured an early stage of the cryptostomata of this plant, which strongly resembles the early stage of *Alaria*. The development of the cryptostoma of *Alaria* is regarded by Mr. Murray as "arrested," and he accounts for this by the fact that the frond of *Alaria* is so thin as not to allow room for the formation of pits. He is confirmed in this view by Mr. Setchell's remark that "the cryptostomata are present on the one-layered portion of the blade (of *Saccorhiza dermatodea*) as clusters of hairs upon the flat surface."

Mr. Murray then goes on to consider *Adenocystis*, a hollow sac-like alga, in form quite unlike any of the other Laminarieæ. He says: "It bears a resemblance both to the cryptostoma of its ally *Saccorhiza* and to those of *Hydroclathrus*, illustrated by Miss Mitchell.

The reproduction of *Adenocystis* is by zoospores, borne in zoosporangia and the cryptostomata occur in the middle of sori of unilocular sporangia." The paraphyses and sporangia, which are truly Laminarian, grow on the very edge of the depression. "In the other Laminarieæ examined, the sorus occurs separately from the cryptostomata, but, in this form, in the middle of the sporangia and paraphyses, as if in a nascent effort (or a dying one, as the case may be), to form a conceptacle like that of *Splachnidium*. It will be remembered that a young conceptacle of *Splachnidium* bears hairs only at first and sporangia later on."

In *Hydroclathrus*, cryptostomata are described by Miss Mitchell as occurring among plurilocular sporangia.

Mr. Murray concludes his paper by alluding to Professor Bower's remark about the groups of hairs in the Dictyotaceæ, "which are the precursors of the reproductive cells." Professor Bower says: "How far these may be compared with the initial cell or hair of the Fucaceæ, it remains for closer observation to decide." Mr. Murray attaches a prophetic value to this utterance, and notes that Falkenberg has pointed out "that in *Scytosiphon* single epidermal cells within a sorus remain sterile and grow out in the form of club-shaped unicellular paraphyses." Hairs are also found in *Asperococcus* and other allied forms, arising in similar situations, "resembling the hairs in the conceptacles and cryptostomata of the Fucaceæ, *Splachnidium*, *Saccorhiza*, and *Adenocystis*, and those in the sorus of Cutleriaceæ and Dictyotaceæ."

In conclusion, I cannot do better than quote here the summing-up of Mr. Murray's interesting paper. "A comparison of the Fucaceous conceptacle and cryptostoma, the Splachnidian conceptacle, with its persistent initial cell and the formation of its hairs yielding place to sporangia, the development of the *Adenocystis* cryptostoma in the heart of its sorus, the other Laminarian cryptostomata (*Saccorhiza* and *Alaria*) apart from the sorus, the cryptostoma of *Hydroclathrus* among its plurilocular sporangia, and finally the cases of the hairs in *Asperococcus* and the Cutleriaceæ and Dictyotaceæ—a comparison of these cases and of the evidence plainly furnished by them points very significantly to a possible origin of cryptostomata." These are striking admissions by one who has heaped contempt on "the ancestors of the Fucaceæ."

Such is the present state of our knowledge of the history and development of cryptostomata. Links are still needed to complete the chain of evidence which is to connect the hairs growing out from the surface of the Dictyotaceæ with the well-developed conceptacles of the higher forms of the Fucaceæ. But there are types still to be examined, and much interesting work to be done. The foreign forms of the Laminarieæ may, and probably will, show variations and gradations in the scale of development hitherto unsuspected, and we may look forward to most interesting results when any worker

examines and describes the sori of these forms in the light of what has been already done.

The questions to be answered, if possible, appear to me to be these :—

Are the Fucaceous conceptacles and cryptostomata the descendants of similar bodies evolved by ancestors approaching the Laminarian type, in which we see them now dying out? Is the Laminarian sorus a mere flattened-out conceptacle? or, have such shelters for spores, antheridia and oogonia, for they are merely that, been evolved independently of each other by these different types under a common necessity of protecting their spores, etc.? Do we now witness nascent or dying efforts to produce conceptacles in such forms as *Adenocystis* and *Hydroclathrus*? Are the barren cryptostomata of *Saccorhiza* prophetic or ancestral structures?

Their further examination, at all events, cannot fail to throw light where it is much needed—on the relationships of the Natural Orders of Phæophyceæ.

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E. S. BARTON.

VII.

Scientific Volapuk.

WE have more than once alluded in these pages to the misapplied ingenuity of certain scientific writers, who, being unable to use the language with which their natural heritage has supplied them, imagine that they can better convey their ideas to the world through the medium of words that are not merely new and strange, but often incorrectly constructed and far too vaguely defined (NATURAL SCIENCE, vol. ii., p. 6, and vol. iii., p. 242). Let it not for a moment be supposed that we deny the value and even the necessity of scientific terminology; on the contrary, for technical purposes, a distinct and well-defined term is, we consider, far superior to an ordinary word distorted from its ordinary meaning. At the same time, it is obviously desirable that these scientific and technical terms should be constructed according to etymological principles of a higher grade than those that govern the appellation of the latest patent medicine or the newest thing in window-blinds. And, apart from this, there are other cautions that might profitably be attended to by many of these word-mongers.

Chiefly should one hesitate before encumbering—we will not say the English language, but—the Volapuk of scientists, with any additions to its already overcrowded vocabulary. Reflect first if the idea cannot be as clearly expressed by words already in use. Brevity may brighten wit, but no less does it darken wisdom. Next consider whether the idea be actually new, or, if new, whether it be a reality. Does, for instance, Mr. Ryder or anyone else seriously suppose that there is an abstract formative force concerned in modifying the forms of bones? If not, where is the necessity for a name? As for those people who go out of their way to invent terms which neither they nor anyone else will probably ever use again in this world, what torture in the next world would be appropriate? Yet this is what Professor Hyatt does in his paper on Bioplastology, when he suddenly stops to tell us that if Hering were right in supposing heredity to be a form of memory, then *Mnemegenesis* would be a very good word for heredity. Shall we suggest that Professor Hyatt should be set first to suppose forms of punishment, and then to invent Greek names for them?

So many new terms have been proposed in the last few years, that we have determined to make some attempt to collect them year

by year in a form available for the use of the scientific public, adding definitions, too often forgotten by the original authors, and derivations when we have been able to discover them. It is not likely that our list for this year is complete. We have purposely taken no notice of terms dealing only with limited groups of animals, and we have not thought it necessary to include every possible combination of terms with prefixes and suffixes suggested by authors. Thus in his paper on Bioplastology (3), Professor Hyatt also subdivides each of the stages of Ontogeny, from Nepionic (=Brephic, Buckman and Bather) to Gerontic, into three substages, which are distinguished, on Buckman's suggestion, by the prefixes *ana-* (up towards), *meta-* (in common with), *para-* (beyond). Thus the Gerontic stage, instead of having two substages, Clinologic and Nostologic (Hyatt), or Catabatic and Hypostrophic (Buckm. and Bath.), will have three, viz., *Anagerontic*, *Metagerontic*, and *Paragerontic*. Professor Hyatt also proposes to apply Haeckel's physiological terms, or his own modifications of them, to Phylogeny, by using the prefix *phyl-*; thus, *Phylanaplasis*, *Phylometaplasis*, and *Phyloparaplasis*.

Besides these, other terms have doubtless escaped our notice, and we shall be glad of any additions. Authors would also confer a favour by sending us, in future, a note of any new terms they may propose.

We cannot conclude without protesting against a tendency, far too prevalent, of introducing these and similar terms, originally invented for the benefit of specialists, into works of a more popular description. This is not the way to popularise science. Possibly the writers think that long words give an air of learning. We believe that they tend to mystify both writer and reader, substituting sound for sense, and, too often, fancy for fact. It was to politicians, not to scientists, that words were given for the purpose of concealing thought; yet we fear that some day the latter also may find that they have floated too far into the clouds of unintelligibility, and may be brought back to earth only to repeat, in the words of the man they shouted "Hi!" at, in the *Hunting of the Snark*—

> "I said it in Hebrew—I said it in Dutch—
> I said it in German and Greek:
> But I wholly forgot (and it vexes me much)
> That English is what you speak!"

List of New Terms Proposed in the Natural Sciences during 1893.

Aborad and *aboran*, converse of "orad" and "oran." (Schulze.)

Astrokinetic (ἄστρον, a star; κινητικός, setting in motion), applied to phenomena of motion of centrosomes of cells. (Ryder.)

Astrosphere (ἄστρον, star; σφαῖρα, sphere), improved term for "attraction sphere," a phenomenon of cell-division. (Strasburger.)

Astrostatic (ἄστρον, a star; στατικός, causing to stand still), applied to phenomena of rest of centrosomes of cells. (RYDER.)

Autotrophic (αὐτός, self; τροφικός, nourishing), existing without aid of commensals: applied to lichens. (MINKS.)

Axian, axial, axiad, adjectival forms of axis, *i.e.*, the longitudinal or main axis. Compare "Centrum" and derivatives. (SCHULZE.)

Bathmology (βαθμός, a step; λόγος, science), the science of growth. A term proposed as alternative to "Auxology" (Buckman and Bather), and correlative of "Bathmism" (Cope). (HYATT.)

Bilateralien. Organic bodies with three axes of symmetry, viz., *isopol*, which equals the perlateral; *heteropol*, or dorso-ventral; and heteropol or longitudinal, the main axis. (SCHULZE.)

Bioplasmology (βίος, life; πλάσμα, substance; λόγος, science), cellular biology; correlative of "Bioplasm, etc." (Beale.) (HYATT.)

Bioplastology (βίος, life; πλαστός, moulded; λόγος, science), the study of the correlations of ontogeny and phylogeny. An alternative to "Biogeny" (Fiske) and "Biogenesis" (Haeckel non Auctt.) (HYATT).

Blastokinesis (βλαστός, germ; κίνησις, movement), migrations of embryo through and over yolk, as in the Orthopterous insect *Xiphidium ensiferum*. (WHEELER.)

Centrad or *proximad*, directed towards the Centrum. (SCHULZE.)

Central or *proximal*, near but not in the Centrum. (SCHULZE.)

Centran, within the Centrum. (SCHULZE.) In the polar axis of a Synstigma. (HYATT, 4.)

Centren, equivalent to "centran" (Schulze) (HYATT, 4.)

Centrum, the imaginary geometrical centre of a Synstigma. (SCHULZE.)

Cephalan, cephalal, cephalad, adjectival forms from κεφαλή, head. Compare "Centrum" and derivatives. (EDITOR in HYATT, 4.)

Co-type (erroneously for "syntype," *cum*, with; τύπος, type); applied in zoology to one of two or more specimens together forming the basis for the original determination of a species. (THOMAS.)

Ctetic (erroneous application of κτητικός, acquisitive), a term for "acquired characters." (HYATT.)

Ctetology (erroneously for "ctematology," κτητός acquired; λόγος science), the study of the effects of environment on organisms, and of the inheritance of those effects. (HYATT.)

Cytokinetic (κύτος, a cell; κινητικός, setting in motion), phenomena of motion within plasma or cells. (RYDER.)

Cytostatic κύτος, a cell; στατικός, causing to stand still), statical conditions within plasma or cells. (RYDER.)

Dextran, dextral, dextrad, adjectival forms from *dexter*, right; referring to right half of perlateral axis of "Bilateralia." Compare "Centrum" and derivatives. (SCHULZE.)

Dictyosome (δίκτυον, net; σωμα, body), thread-like bodies appearing in cell-division, *e.g.*, spermatogenesis of *Branchipus*. (MOORE.)

Diplanetism (apparently an erroneous form of *diplanesis*, from δι's, twice, and πλα'νησις, a dispersing), double swarming of zoospores (HUMPHREY.)

Distad, directed from the Centrum. (SCHULZE.)

Distan, on the periphery of a Synstigma. (SCHULZE.)

Dorsan, on the outer surface of the dorsum. (SCHULZE.)

Ectergogenesis (ἐ'κτό's, without; ἔ'ργον, work; γέ'νεσις, birth), the passive reactions of an organism to environment. (HYATT.)

Ectergogenic (erroneously for "ectergogenetic"), adjectival form of the above. (HYATT.)

Entergogenesis (ἐ'ντό's, within; ἔ'ργον, work; γέ'νεσις, birth), the active reactions of an organism to environment. Equals "effort" of the Neo-Lamarckians. (HYATT.)

Entergogenic (erroneously for "entergogenetic"), adjectival form of the above. (HYATT.)

Entergogenism (an impossible word), the principle of the above. (HYATT.)

Ergogeny (erroneously for "ergogenesis") (ἔ'ργον, work; γέ'νεσις, origin), general term for all forms of energy producing modifications of bone. (RYDER.)

Ergogenetically adverbial form of the above. (RYDER).

Genesiology (γέ'νεσις, birth; λο'γος, science), the study of the phenomena of heredity. (HYATT.) This term has already been used in another sense, viz., "the science of generation," and should therefore be inadmissible.

Genetic force, the active force of heredity. (HYATT.)

Genism (an impossible word), the principle of heredity. (HYATT.)

Hemera (ἡ'μέ'ρα, day), a chronological subdivision of the geological term "age," marking the acme of development of one or more species. (BUCKMAN.)

Heteropol, see *Bilateralien*.

Heterotrophic (ἕ'τερος, other; τροφικό's, nourishing), existing with aid of commensals; applied to lichens. (MINKS.)

Isopol, see *Bilateralien*.

Karyokinetic (κα'ρυον, a kernel; κινητικό's, setting in motion), applied to phenomena of active stages of cell-nuclei, an old term restricted. (RYDER.)

Karyostatic (κα'ρυον, a kernel; στατικό's, causing to stand still), applied to phenomena of resting stages of cell-nuclei. (RYDER.)

Kinetogeny (erroneously for "kinematogenesis," or "kinesigenesis," κινεῖν, to move; γέ'νεσις, origin), energy of motion, the agency producing modifications in the form and proportions of bones. (RYDER.)

Meta-type (μετα', after; τύ'πος, type), applied in zoology to a specimen collected at the exact locality whence was obtained the type of the species to which it belongs, and named by the author of that species. (THOMAS.)

Mnemegenesis (μνήμη, memory; γένεσις, descent), Hering's explanation of heredity as a form of unconscious organic memory. (HYATT.) It will be observed that, in this compound, "genesis" is used in a different sense to that which it bears in the rest of Hyatt's coinage.

Monocyclon, or *Monocyclic*, is applied to a phylogeny in which only one cycle is observed. (HYATT.)

Morphic (μορφή, form), connected with structure. Instead of morphologic, when incorrectly used in this sense. (HYATT.)

Nepiastic, an emendation of the corrupt "nepionic," or "næpionic," of Hyatt; derived from νήπιος, infant, presumably by way of the rare form, νηπιάζω, I play like a child. It equals "Brephic" of Buckman and Bather. (SCHUCHERT.)

Nostic (erroneously formed from νοστός, a return home), the final stage of decline. Supplants "Nostologic" (Hyatt), and "Hypostrophic" (Buckman and Bather). (HYATT.)

Ontocycle or *Ontocyclon*, the cycle of individual development. (HYATT.)

Orad, towards the mouth opening, when mouth and anus are terminan and opposite. (SCHULZE.)

Oran, within the mouth opening, under the same conditions. (SCHULZE.)

Paraplasis, decline in Ontogeny, senescence. Supplants "Cataplasis" (Haeckel). (HYATT.)

Parameridian (a mongrel term from παρά and *meridianus*), applied to any plane parallel to the meridian or plane of the principal axis in a Syngramma. (SCHULZE.)

Paratangential (a mongrel of the same litter), within the body of a Synstigma and at right angles to its radii. (SCHULZE.)

Para-type (παρά, alongside of; τύπος, type), applied in zoology to a specimen of the original series forming the basis for the original determination of a species, but not the type, in cases where the author has himself selected a type. (THOMAS.)

Peripheran, on the periphery of a Syngramma. (HYATT, 4.)

Phylocycle or *Phylocyclon*, the cycle of development of the race. (HYATT.)

Polycyclon or *Polycyclic* is applied to a phylogeny in which many cycles are observed. Further, the number of cycles may be expressed: thus, the Arietidæ, having ten cycles, are *Decacyclic*. (HYATT.)

Pseudosomes (ψεύδος, false; σῶμα, body), bodies corresponding with centrosomes in everything except number, which appear in spermatogenesis of *Branchipus*. (MOORE.)

Sinistran, *sinistral*, *sinistrad*, converse of *dextran*, etc. (*q.v.*). (SCHULZE.)

Statogeny (erroneously for "stasigenesis," στάσις, a standing; γένεσις, origin), antithesis of *kinetogeny*. (RYDER.)

Sympeden = Bilateralien. (SCHULZE.)

Syngramme (σύν, with; γράμμα, a drawing), organic bodies symmetrical about a main axis. This term supplants "Centraxonia" (Haeckel). (SCHULZE.)

Synstigmen (σύν, with; στίγμα, a mark), organic bodies with an imaginary geometrical centre. This term supplants "Centrosigmen" (Haeckel). (SCHULZE.)

Syntrophy (σύν, with; and τρέφω, I nourish) with erroneous variant *syntrophism*, and substantive *syntroph*. Apparently equals "commensalism" of former authors, but is specially applied to the endophytic elements of lichens. (MINKS.)

Tangential, at right angles to the radii of a Synstigma, and on the surface thereof. (SCHULZE.)

Termini, the ends of the main axis of a Syngramma, with adjectival forms *terminal*, *terminan*, *terminad* (compare "Centrum" and derivatives). (SCHULZE.)

Topo-type (τόπος, place; τύπος, type) applied in zoology to a specimen collected at the exact locality whence was obtained the type of the species to which it belongs. (THOMAS.)

Ventran, on the outer surface of the venter. (SCHULZE.)

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SOME NEW BOOKS.

ANNALS OF BRITISH GEOLOGY, 1892. A Digest of the Books and Papers published during the Year with Occasional Notes. By J. F. Blake, M.A., F.G.S. Pp. 310, 8 plates, and text illustrated. London: Dulau & Co., 1893. Price 10s.

THIS is the third volume of an extremely useful work, the previous volumes of which were noticed in NATURAL SCIENCE (vol. i., p. 69, and vol. ii., p. 234). The labour of preparing such a record is so enormous that we should under any circumstances have offered our thanks to Mr. Blake; but the present volume is so great an improvement on its predecessors that we must also tender him our congratulations. The work has passed through the fire of criticism and has emerged all the better for its ordeal. It has occasionally been thought that Mr. Blake was too unwilling to distinguish between the functions of annalist and analyst. The pungent notes that he used to interject in the middle of an abstract sometimes caused as much wrath as merriment. These are now absent, to the delight of authors if not of readers. Not but what our recorder still has his say, only his criticisms are now placed together in the form of an introductory review. This, he thinks, can scarcely be objected to if we admit the principle that "thou shalt not muzzle the ox that treadeth out the corn." To judge from this review the authors also seem to have improved, at least in Mr. Blake's opinion, and we do not think any of them are likely to reply, in the words of Agamemnon's watchman,—

"For the rest I must be silent, a great ox hath trod upon my tongue."

The abstracts now seem still more carefully done, and if they do not satisfy authors, then the authors have only themselves to blame, since they are now afforded the opportunity of revising the manuscript itself, and this is a privilege of which we are glad to see the majority have availed themselves. The illustrations are also greatly improved, partly owing to original blocks having been lent by some societies and individuals, a mode of assistance which might very well be extended. It is, however, to be hoped that the wretched appearance of fig. 2 is not due to the fact of the author having changed his printer.

Except for the addition of the interesting introductory review, which the authors mentioned will save us the labour of criticising, the arrangement of the volume follows the old lines. One or two papers, however, seem to have gone astray. The Cave Men of Mentone, for instance, were hardly Britishers, while Mr. Teall's paper in NATURAL SCIENCE on the "Sequence of Plutonic Rocks" took its examples from foreign rather than home sources. The inclusion of a section dealing with papers on foreign geology published in England is probably intended as a bait for foreign purchasers;

personally we think that in abstracting these papers, the compiler gives himself unnecessary trouble.

We have now done our duty by this book. It only remains for our readers to order copies, and so to give Mr. Blake that pecuniary support without which work of this kind cannot be carried on. If this support is not given, "the only remaining hope," says Mr. Blake, "would be that the Geological Society should undertake a record at their own expense, by the aid of paid recorders." A society so poor that it cannot subscribe £100 *per annum* for the recording of palæontological literature, is hardly likely to jump at this suggestion. On the other hand if support is forthcoming, then, says Mr. Blake, the "Annals" might very well be expanded to the "Annals of British and *Foreign* Geology."

It may be pointed out that records of geological literature are now published in most European countries and to some extent in America. Why, in the name of common sense, cannot the societies and individuals engaged in carrying on this most valuable work combine their scattered and often wasted energies, and issue a series of annual bibliographies for each country of the world? Each such bibliography might contain either the papers published *in* its own country or those *about* its own country. The former plan would be cheaper, easier, more likely to be complete, and in the end more useful. The labourers are there; they only want a leader. *Verb. sap. sat.*

F. A. BATHER.

THE STORY OF OUR PLANET. By T. G. Bonney, D.Sc., LL.D., F.R.S. Medium 8vo. Pp. xv., 592, with six plates and 170 illustrations. London: Cassell & Co., 1893. Price 31s. 6d.

THIS work forms a companion volume to Sir Robert Ball's "Story of the Sun." It is planned, as the author himself states, somewhat on the lines of Lyell's "Principles of Geology," and it seems a pity that so excellent a model is not more closely followed. Professor Bonney divides the book into five sections, entitled: "The Story: its Books and their Speech," "The Processes of Sculpture and Moulding," "Changes from Within," "The Story of Past Ages," "On some Theoretical Questions." The first part contains introductory chapters on geology and physical geography, such as can be found in any text-book; but they do not seem to be carefully welded together, or to form parts of a connected whole. The second part is better done, and the author treats in a thoroughly systematic manner of the changes that take place on the earth's surface. Whenever it speaks of mountains, or of phenomena occurring among mountains, the book becomes interesting, and the illustrations also are better selected. The parts relating to glaciers, for instance, are very good; though the author would, perhaps, have spoken less confidently as to origin of lake-basins through irregular subsidence if he had seen Mr. A. R. Wallace's paper in the last *Fortnightly Review*. Leaving out of account theoretical questions, upon which opinions may differ, we have not noticed many serious mistakes or omissions in this part of the book. Fig. 34, fossil rainprints, if it represents anything of the sort, which we doubt, must represent, not the rainprint itself, but a cast of the print seen from below.

The third part, "Changes from Within," contains accounts of earthquakes and volcanic eruptions, including several important ones that have taken place since the publication of the last edition of

Lyell's "Principles." It also gives an outline of certain recent discoveries in mountain structure, and on the origin of the crystalline schists; though this part is scarcely up-to-date.

The fourth part commences with an account of meteors and the earth's beginning, leading to chapters on historical geology. All this section seems to be compiled from various well-known text-books, and would be improved by more thorough revision. The final section, "On some Theoretical Questions," contains four disconnected chapters, which we cannot help thinking would be better placed in the sections to which they properly belong, for other equally theoretical questions have been discussed in various parts of the volume. The four chapters thus isolated refer to "The Age of the Earth," "The Permanence of Ocean Basins and Land Areas," "Climatal Change: its cause and history," and "The Distribution and the Descent of Life."

We have, perhaps, criticised this book somewhat more severely than we should otherwise have done if the author had not challenged comparison with Lyell. The mantle of Lyell has not descended to Professor Bonney; but if unscientific readers wish for a popular "Story of our Planet," we can safely recommend the present work.

Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs. Band. vi., Abtheilung iv. Aves. II. Systematischer Theil. By Hans Gadow, Ph.D., F.R.S. 8vo. Pp. 303.

Dr. Gadow, in the concluding parts of the above-mentioned work, adds the keystone to his long labours, which, it is not too much to say, will prove of incalculable benefit for all time.

Those who are familiar with the anatomical section of this book will naturally turn to the present volume with great expectations; and assuredly they will not be disappointed, for they will find that, what has ever proved a subject of extraordinary difficulty and complexity, whether regarded from the limited horizon of the Ornithologist, or the more extended range of the Biologist, has been treated with a thoroughness that will claim at once the gratitude and the admiration of all workers in this field.

By reason of the plan adopted by Dr. Gadow of presenting us with tables of comparison between the various orders and families and pithy "conclusions," it is possible to trace, step by step, the ground over which the author has passed in the construction of his present scheme—a boon which we shall not sufficiently appreciate till we recollect the painful groping in the dark, and fruitless pursuit of phantom probabilities, that must necessarily have been involved in a research of this nature.

Substantially, the present arrangement is the same as that which appeared in the *Proc. Zool. Soc.*, 1892, enlarged and improved.

Thus the Class Aves is now divided into two sub-Classes, Archæornithes and Neornithes; and three divisions, "Neornithes Ratitæ," "Neornithes Odontolcæ," and "Neornithes Carinatæ." The third division is divided into two brigades of two "legions" each, the Colymbomorphæ and Pelargomorphæ comprising the first, and the Alectoromorphæ and Coraciomorphæ the second brigade. To the Colymbomorphæ belong the Ichthyornithes, Colymbiformes, Sphenisciformes, and Procellariiformes: while the Pelargomorphæ embrace the Ciconiiformes, Anseriformes, and Falconiformes. The Alectoromorphæ contain the Tinamiformes, Galliformes, Gruiformes, and

Charadiiformes; and the Coraciomorphæ, the Cuculiformes, Coraciiformes, and Passeriformes.

Such are the main divisions of Dr. Gadow's scheme. To pursue it further, would be neither permissible nor desirable here; but we think, in justice to our task, we should indicate some of the principal features of the remainder of the book; which, as we have previously hinted, is simply teeming with information, not of the "handy reference-book" order, but facts, skilfully marshalled.

Dr. Gadow commences with a short definition of the Class Aves, in which he admits the derivation of birds from reptiles; though from which sub-class cannot of course be proved. We are reminded that the distinction between birds and "not-birds" is unmistakable; yet it is only a question of time for some fortunate palæontologist to bridge over the gap, since gap there must be, for such remarkable structures as feathers, and the modification of the fore-limb into the characteristic Avian wing, cannot have come suddenly into existence. *Archæopteryx*, the most archaic bird we know, has attained a high degree of development; so that there must have existed a long chain of unknown forms connecting it with the reptiles. For such intermediate forms the term "Herpetornithes" is proposed.

The Neornithes are probably the direct descendants of the Archæornithes; but whether *Archæopteryx* was a direct ancestor, or only a small side branch, is a matter for speculation. The Neornithes seem to have become well developed by the end of the Jurassic period; in Cretaceous times they still retained teeth, but had even then developed into flying forms with a pygostyle, and flightless forms without either a carina sterni or pygostyle.

After discussing the characters, whether primitive or pseudo-primitive, which distinguish the Ratitæ from the remaining birds, Dr. Gadow proceeds at some length to dilate upon the relation of the Ratitæ to the Carinatæ. He opens his argument by very properly disposing of the mythical relationship, hitherto considered by some to exist between the Ratitæ and the Dinosaurs on the one hand, and the Carinatæ and Pterosaurs—through *Archæopteryx*—on the other, as "profitless dreaming"; pointing out that, if the similarity of the hind-limb existing between certain of these reptiles (the Dinosaurs) and the Ratitæ be held to be a proof of relationship, then all birds must be derived from the same stock, since in all birds the component elements of the tarsus are the same. Proceeding, the author next discusses the mono- or polyphyletic derivation of the Ratitæ, and this section will require very careful reading. Dr. Gadow's conclusions have led him to regard the Ratitæ as monophyletic. He proposes to retain the "Ratitæ" as a separate group, "upon practical taxonomic grounds."

The division "Neornithes Odontolcæ" appears to have been made with some diffidence to contain the Hesperornithes and Enaliornithes; but, it is urged, the arrangement must remain until Jurassic or Cretaceous birds are discovered, which possess a carina sterni and teeth standing in a groove.

The third and last division—"Neornithes Carinatæ"—contains all the remaining birds. Of brigade 1 it is noteworthy that the Colymbomorphæ (= legion 1) include the Ichthyornithes—and, we presume, but for special reasons, the Hesperornithes would have been included also. Passing on to the Coraciomorphæ, for space will not permit us to slowly meander as we should wish through the intermediate groups, we notice that the author confirms his earlier paper

both in placing the Hornbills as a sub-family of the Hoopoes (Upupidæ), and retaining the Striges in close proximity to the Caprimulgi, instead of reverting to their time-honoured position, with the Accipitres.

Both cases will, we anticipate, create some discussion. In the case of the Striges, we fail to see how anyone, after he has carefully and impartially gone through the mass of facts which have been collected, as if for the very purpose of converting sceptics (pp. 235-241), can conscientiously stand by the old arrangement.

"The division of the Oscines into families is as yet very unsatisfactory," and we are referred for the present to the British Museum catalogues. The Australian region is to be regarded as the birthplace of the Oscines, from whence they have overspread the whole habitable earth.

Towards the end of the book some fifteen pages have been devoted to the Geographical Distribution of Birds, and extremely interesting reading it proves. The systems of Sclater, Wallace, Huxley, Newton, Sharpe, and others, are briefly reviewed, and then follows a short "Examination of birds as to their utility for dividing the earth into ornithological regions," which is fitly terminated by a contribution from Dr. Gadow himself, in which he recognises two main regions, A—Notogæa, and B—Arctogæa; the former embracing an Australian and Neotropical region, the latter a Palæotropical and Periarctic region.

Now that Dr. Gadow has said his last word, the question for the Ornithologist in the future will be not how much *is*, but how much is *not* known on any particular question upon which he may require information. The author is to be congratulated on the way in which he has fulfilled his most difficult and laborious task. W. P. P.

HORNS AND HOOFS; or, Chapters on Hoofed Animals. By R. Lydekker. 8vo. Pp. 411, with 82 woodcuts, London: Horace Cox, 1893. Price 15s.

THIS is a popular work, comprising a series of articles originally contributed to the *Field* and *Land and Water*, and now reissued in collective form, with some revision. The volume is beautifully printed and well illustrated, and will prove of interest not only to the sportsman, for whom it is primarily intended, but also to the naturalist who happens to be unfamiliar with the latest discoveries and most advanced views in reference to "Big Game" of all kinds except elephants and Carnivora. The first chapter deals with the Wild Oxen, which are all included by Mr. Lydekker in the genus *Bos;* and the various groups are described under the headings (1) aurochs and zebus, or true oxen; (2) gaur; (3) yak; (4) bison; and (5) buffalo. The musk-ox (*Ovibos*) is placed separately at the end. The second chapter treats of the Wild Sheep, which are essentially mountain animals, and are especially numerous in the highlands of Central Asia. They are subdivided into (1) the bighorns of North America and Kamschatka; (2) the argalis of Central Asia; (3) the urials of Asia, with the moufflons of Asia Minor and Europe; and (4) the bharals, as represented by the true bharal of Little Thibet and the Barbary sheep of North Africa. The Goats are next discussed and arrayed in five groups, as follows:—(1) The ture of the Caucasus, with the so-called Spanish Ibex, the sheep-like goats; (2) the pasang, or true goats; (3) ibex; (4) markhor; and (5) the tahr, or *Hemitragus*, of India. The Antelopes and Deer are

treated at great length, incorporating in a general way the latest results of systematic zoologists; and the volume concludes with two chapters on Pigs and Rhinoceroses. Unlike most authors who furnish Zoology for the general reader, Mr. Lydekker makes numerous allusions to the extinct forerunners of the "Big Game" he describes; and there is a pleasant tinge of philosophy in "Horns and Hoofs" which one rarely meets with in works of its class.

CONCHYLIOLOGIE FRANCAISE. LES COQUILLES DES EAUX DOUCES ET SAUMATRES DE FRANCE. By A. Locard. 8vo. Pp. 327, with figures in text. Paris: J. B. Ballière & Son, 1893. Price 18 frs.

THE present volume is one of a series by the same author on the shells (not the mollusca, be it noted) of France. Its scope is sufficiently indicated if "recent and quaternary" be added to its title. The work is an admirable example of the modern French school at its very worst, and is eminently calculated to dishearten any student who should attempt to use it, and cause him to abandon all pursuit of the subject in disgust and despair. The figures are, compared with the number of "species" described, few and poor; the descriptions are inadequate and confusing; while the whole work is superficial to a degree. Every slight variation in the form of the shell is held sufficient ground for making a distinct "species," and as such most synonyms and bad varieties are consequently ranked. The rows and rows of these under *Lymnæa*, *Planorbis*, *Pisidium*, and *Unio* are, for instance, bad enough, but the author fairly "beats the record" under *Anodon*. Where Isaac Lea, who was no mean hand at species-making, could only see one, Locard makes two genera with 279 species! Further comment is needless.

BRITISH EAST AFRICA OR IBEA: A History of the Formation and Work of the Imperial British East Africa Company. By P. L. McDermott. Pp. xvi., 382, with map. London: Chapman & Hall, 1893. Price 6s.

AT a time when the system of the extension of the Empire by means of Chartered Companies is on its trial, this history of one which has had a rather unfortunate career is very opportune. The book is almost entirely political, and is an *ex parte* statement of the case for the Company against its foes, both official and unofficial. It gives but little information about the country; what little there is has been written in such an optimistic spirit, and the statements are so vague and general, that one may be excused for distrusting it. One could have wished that some of the lengthy quotations from leaders in *The Times* had been replaced by extracts from the unpublished reports of the various experts, geological, agricultural, and mining, who have been employed by the Company. As the future of the district must largely depend on its economic resources, the public would be in a better position for judgment had such materials been placed before it instead of the story of the Company's past discussions with the Foreign Office. As the work has been written by a paid official of the Company at the instance of its chairman, we have no right to expect impartiality and the insertion of matter that is not to the credit of the Company; and we have not got it. For example, we are told about the station at Dagoreti as if it were still flourishing and in existence; as a matter of fact, it was abandoned by the Company's officers and immediately burnt by the Wakikuyu some years ago, and

a new one has since been built at Kabeti to replace it. Mr. McDermott has certainly done his work well, and given us an interesting history and ingenious defence of the Company; but we fear he will only strengthen the general feeling that the Company's record shows that a mixture of philanthropy and business seldom pays. Had the Company stuck to one or the other, and especially if it had confined its attention to administration pure and simple, and the utilisation of the available resources of the district, this book would probably never have been written, for an apology might then have been unnecessary.

REPORT OF THE SECRETARY OF AGRICULTURE, 1892. 8vo. Pp. 656, with 71 plates. Washington: Government Printing Office, 1893.

ON the back of the title-page of this somewhat bulky volume there is printed a copy of a public resolution:—"Resolved by the Senate and House of Representatives of the United States of America in Congress assembled,—That there be printed 500,000 copies of the Annual Report of the Secretary of Agriculture for the year 1892"; and also "That the sum of 300,000 dollars, or so much thereof as may be necessary, is hereby appropriated . . . to defray the cost of printing said report."

Three hundred thousand dollars is, roughly, £60,000. Our Transatlantic cousins have a vastly higher opinion of agriculture as a national concern than we ourselves if they are willing to spend £60,000 in taking the first step to make public the results of one year's work. But then, as Mr. Rusk, the Secretary, observes, "The great blessing which this country enjoys from the fact that it is far less than some other countries the home of large landed proprietors," presents certain difficulties which it is the province of the Department to remove. That is to say, the absence of large landowners, commanding extensive capital, throws the burden of experiment and investigation of agricultural problems on the shoulders of the Agricultural Department. This, of course, explains the bulky volume, the perusal of which we would recommend to large landowners in general, as an incentive to the publication of those results of experiment and investigation which we have a right to expect from the possessors of extensive capital.

The general report of the Secretary occupies sixty pages, and gives an idea of the scope of the Department and the possibilities of future development. Unfortunately, we find it hampered by "the inadequate compensation which it is authorised to offer to the men of talent, scientific education, and experience which it needs to carry on its most responsible duties," in which its "facilities will be found to compare very unfavourably with those of the other Departments of the Government." We know of parallel cases. It does seem somewhat incongruous that, while 300,000 dollars are voted for printing reports, the herbarium, including the largest collection of American grasses in existence, and containing type specimens of nearly all the American species described during the last fifteen years, should be housed in a non-fireproof building. Meanwhile "Congress saw fit to extend an appropriation, covering the present fiscal year," to provide for experiments on the production of rainfall by explosives, but Mr. Rusk does not think that his facts relating to this subject justify the anticipations formed by the believers in this method of artificial rain-making.

After a short report by the Assistant Secretary on the divisions directly under his charge, follows a series of detailed reports by the specialists in the various divisions. These are the Chief of the Bureau of Animal Industry, the Chemist, the Entomologist, the Ornithologist and Mammalogist, the Botanist, the Chief of the Division of Vegetable Pathology, the Pomologist, the Microscopist, the Chief of the Division of Forestry, the Special Agent in charge of Fibre Investigations, the Superintendent of Gardens and Grounds, the Statistician, the Chief of the Seed Division, the chiefs of various publication and financial divisions, the Director of the Office of Experiment Stations and the Chief of the Weather Bureau. Finally, there is a special report on tea-raising in South Carolina.

We cannot discuss these reports individually. They certainly contain a great deal of interesting and valuable matter. Thus, that of the Chemist begins with a warning against black pepsin, a preparation of pepsin and sugar, designed to increase the yield of butter, but which merely gives a compound which is not the genuine article, but an incorporation of butter with about an equal weight of other substances. An extensive examination has been made of canned and preserved foods, and it is pointed out that in some cases the tin employed to coat the cans contained over 10 per cent. of lead, which, in contact with the natural acids of fruits and vegetables, might lead to the formation of lead salts highly injurious to health. Particular attention was paid to the examination of green-coloured peas, beans, and other vegetables. It is a common custom to use a copper salt, presumably the sulphate—in canning such materials, to preserve or intensify their green colour, and the consumer may be certain, when eating very green canned peas, beans, etc., that he is consuming large quantities of copper, which, as the Chemist observes, must be condemned on hygienic grounds. This report also gives information on the detection of adulteration in honey, and describes experiments with sugar beets and sorghum. The Entomologist discusses various insect pests, such as the potato-tuber moth, the ox warble, the rose saw flies, and others, and gives an account, illustrated by three plates, of the Insectary.

The report of the Pomologist contains some brightly-coloured plates, depicting varieties of different fruits suitable for cultivation. The Eldorado blackberry looks very luscious. The Microscopist figures some edible mushrooms and also gives methods for their culinary preparation and simple means for detecting poisonous species. And so on; but we have said enough to give an idea of the contents of the report and for further details must refer the inquirer to the original.

ILLUSTRATED GUIDE TO BRITISH MOSSES; with Keys to the Genera and Species. By H. G. Jameson, M.A. Published by the Author, 6 College Road, Eastbourne. 8vo. Pp. 80, with 59 plates. Price 7s. 6d.

WE are always glad to welcome a good book dealing with any phase of British botany. Questions of general morphology, anatomy, physiology, and the like occupy so large a proportion of the energies of the modern botanist that the flora of his own country is neglected or left to workers whose want of opportunity, training, or ability often forms a serious hindrance to success. Happily, the present is not a case in point. It is evident from his preface and the style of the whole work

that Mr. Jameson has himself felt a want, and then done his best to supply it for the benefit of others. The scope of the book is well expressed in the title. It does not profess to take the place of such works as Hobkirk's synopsis, but to meet the difficulty of the beginner, who is often at a loss to know whereabouts in these books he is to look for the description of his unknown and unnamed specimen. By the aid of carefully elaborated keys he arrives first at the genus and then at the species, and the specimens once identified, can be studied in detail in the larger works. The keys hitherto published have been founded almost entirely on fruit characters, which, as the author observes, leave the student quite at a loss with regard to some of the commonest and most easily distinguished mosses, which will probably be among the first gathered, but are not likely to be in fruit. By the present arrangement, however, most of the Pleurocarpous mosses and many of the Acrocarpous may be readily determined even in the barren state. These keys were originally published in the *Journal of Botany* during the year 1891, but have been thoroughly revised and in great part re-written for the present work.

The first 20 pages of the guide are devoted to a short but useful introduction. This, with the help of the illustrations, comprised chiefly in Plates I.–VII., to the figures in which ample reference is made, forms an excellent glossary, besides giving a good general idea of the external structure of a moss. In section VII., on the practical examination of specimens, the beginner will find all necessary instructions. Under each genus in the keys to the species, the author gives some introductory remarks which, while not professing to be a generic description, call attention to points characteristic of the British species, and sometimes include information on synonymy or recent additions to the British Moss Flora.

The great features of the book are the 59 plates, which comprise, in fact, two-thirds of the whole. Mr. Jameson is his own artist and lithographer, and we heartily congratulate him on the result. Drawings of the entire plant are rare, but those characters are given which are especially useful in distinguishing the species, namely, the form of the leaf, leaf-apex, leaf-cell, and often also of the fruit.

We hope the Guide will meet with the appreciation it deserves, and add stimulus to the study of a somewhat neglected but highly-interesting division of our Flora.

THE JURASSIC ROCKS OF BRITAIN. Vol. iii. The Lias of England and Wales (Yorkshire excepted). By H. B. Woodward, F.G.S. 8vo. Pp. xii., 299, with map and woodcuts. Memoirs of the Geological Survey of the United Kingdom. London: Printed for Her Majesty's Stationery Office, 1893. Price 7s. 6d.

IT is with irate feelings that we peruse this book; not, we hasten to add, on account of anything with which the author has connection. Our anger is roused by the "get up" of the volume. Badly printed with inferior type often set awry, on decidedly third-rate paper—material so thin that the printing of one side of the page seriously interferes with the other—the work is neither a credit to its printers nor to the British Government. Placed on our desk by the side of Government publications of other countries, it does much to lower our national pride, and it sets us wondering why our Government should take such pains to show how badly they can publish a work. The book is embellished with woodcuts of sections, which the

printing has made none too clear, and with figures of fossils which, from the same cause, can hardly be identified, notably on p. 44.

In striking contrast to the above are the contents of the volume, which are, for the most part, worthy of the painstaking ability of the author. The chapters on economic geology are undoubtedly the best part of the work; they contain much information valuable to others besides geologists. The stratigraphical portion of the volume displays the writer's general knowledge of the Liassic deposits to the best advantage; and it gives a remarkable amount of information for which every geologist will be thankful. Had this information not been marred by an unfortunate abundance of obvious palæontological inaccuracies, we should have had nothing but praise for this work. Unhappily, a considerable laxity in the use of specific names, and a too great reliance on out-of-date records, has, we regret to say, rendered the palæontological information to a large extent untrustworthy. We need not warn the field-geologist; a very little investigation will tell him that as regards the zonal records, both in the body of the work and in the appendix, it will be safer "to divide by four and disbelieve the quotient." In such case, however, the descriptive portion of the work becomes hardly more than a guide-book to the quarries and to the beds that they contain; but a volume on which so much labour has been bestowed is surely worthy of something higher than this.

It would have improved the work immensely, in our opinion, if the lists of species had been compared with those of good continental authorities, and if records on which doubt was thus cast had been expunged. A healthy scepticism as regards the insular idea that this country can show associations of species which the rest of Europe cannot produce should certainly be encouraged. We may pass the inevitable mistake of calling *A. falcifer* by the name of a generically different fossil, *A. serpentinus;* but such records as *A. semicostatus* and *A. raricostatus* together "above their appropriate zones" (p. 66), *A. subplanicosta* and *planicosta* in the same bed (p. 67), *A. semicostatus* and *subplanicosta* together (p. 165), *A. birchi* and *A. oxynotus* in conjunction (p. 148), and the table at the end of the volume having *Amm. planicosta, raricostatus,* and *semicostatus* ranging from the *Bucklandi*- to the *Jamesoni*-zone, *A. striatulus* from the *Annulatus* to the *Jurensis*-zone, and *Rhynchonella calcicosta* from the *Planorbis* to the *Spinatus*, may well excite suspicion as to the trustworthiness of the palæontology. The specialist, from his knowledge of the labels which *Ammonites* often bear, will no doubt be able to translate some of the above, as, for instance, "*A. planicosta*" with *A. sirius* (Reynès), and "*A. raricostatus*" with the young of *A. sauzeanus* (d'Orbigny); but the general geologist will, no doubt, be fogged.

The wisdom of the division between Lower and Middle Lias adopted in this work, contrary to general palæontological opinion, and contrary to what obtains in the companion Survey volume on Yorkshire, may well be questioned. In order to make the disproportion between Lower and Middle Lias less conspicuous, the author has had recourse to a "lumping" of the zones between those of *Angulatus* and *Capricornus*; while, on the other hand, a more than usually full recognition is accorded to the zones above the latter. As a consequence of the lumping, the zonalist will find in the appendix-tables a curious admixture of zonal species besides those noted above. Perhaps, however, what will most surprise him in this connection is that six species of Ammonites are said to pass from the Lower to the

Middle Lias; while no less than seventeen species are stated to go from the Upper Lias into the Inferior Oolite. This peculiar result is, of course, due to the author's method of treatment of the Upper Lias in Dorset and Gloucestershire; and it will be interesting to note in the Oolitic volume whither the author will be led by this pursuit of lithological uniformity in defiance of palæontological evidence.

From mistakes other than those of identification, and from misprints, the work seems to be creditably free. *Dactyloceras* is given throughout the Appendix instead of *Dactylioceras*; and a species called *Ophioderma milleri* on p. 192 is alluded to as *O. tenuibrachiata* on p. 198; while "mutations of sub-genera" (p. 328) certainly puzzles us.

A certain conservatism has prevented any attempt to classify *Ammonites* according to genera; but it has not been extended to Gasteropods, for new genera like *Pseudomelania* and *Nortonia* are admitted. There is the same conservatism in the matter of Ammonite-species. We are told that "many of the species are so split up that the multitude of names is simply bewildering, and they become of little or no service to the stratigraphical geologist" (p. 328). Now, we were under the impression that from the days of Oppel onwards the best stratigraphical work done on the Continent and in America had been accomplished by those who were strict in specific identification. But here, again, this conservatism is only applied to *Ammonites:* in Brachiopods there are catalogued under *Spiriferina* all those species which Davidson was so doubtful about admitting. This is surely inconsistent.

In conclusion, we must express our sincere regret that a volume which contains an immense amount of valuable work should have been disfigured by palæontological inaccuracies arising from a laxity in the use of specific names. We regret it for the author's sake; because it too often happens that inaccuracies in the part about which he can form an opinion induce the reader to condemn the rest, of which he knows naught. Though excusable, this is often unjust.

S. S. B.

POEMS. By Arthur Christopher Benson. 8vo. Pp. xvi. and 192. London: Matthews & Lane, 1893. Price 5s. nett.

SCIENTIFIC people are often said to have no care for literature. This is surely a libel; at any rate lovers of nature will do well speedily to make the acquaintance of this little volume. In days when so many write for the sake of writing, and to be read by other writers, it is a pure pleasure to chance on a poet who takes us back again to the simple, strong heart of the world without intervention of Grub Street fripperies. Other poets have incidentally cast flashes of light on our ordinary surroundings, making for us beauty where none was seen before; but Mr. Benson takes the common objects of the field as the very text of his songs. Berries of Yew, Old Nests, The Mole, The Dandelion, Knapweed: such are some of the subjects on which he writes with the insight born of love. Not often does he deal with the deeper questions of natural science, but the following lines from the very beautiful poem addressed to A Child touch on the mysteries of inheritance:—

> Old signs are written in thy tender face,
> Desires, regrets that thou hast never known.
> Thou art the heir of thy aspiring race,
> Heir of a troubled throne,

Of hope, that hardly dost portend the morn,
And sadness, that hast scarcely guessed at pain;
God takes the characters of fate outworn,
And writes them fair again.

In his minute observations of nature, Mr. Benson reminds one of Gilbert White, and we venture to quote the following sonnet on the naturalist as of interest to our readers, and as giving some idea of our author's poetic method :—

Thou wast a poet, though thou knew'st it not
Then, on a merry morning, when the thrush
Fluted and fluted briskly in the bush,
And blackbirds whisked along thy garden plot,
Didst watch an hour beside thy hanger's foot
The quivering kestrel hung aloft the skies
To mark aught stirring, or with pensive eyes
In cherry-orchards didst forecast the fruit.
And shall I deem it idle thus to scan
The myriad life, and reverently wait,
A patient learner, auguring, behind
The restless hand, the unhesitating mind?
This was thy daily task, to learn that man
Is small, and not forget that man is great.

Space does not permit us to quote more; but we cannot conclude without offering our thanks to Mr. Benson for the pleasure he has given us. F. A. B.

INDEX TO THE PERIODICAL LITERATURE OF THE WORLD (COVERING THE YEAR 1892). By Miss E. Hetherington. 4to. London: *Review of Reviews* Office, 1893. Price 5s.

THIS valuable reference book has reached its third year and, as Mr. Stead truly says, we owe much to the industry and enthusiasm of Miss Hetherington and her assistants. We do not propose to criticise so useful a book; and, limiting ourselves to our own department, we offer a few suggestions for the next year's issue. In the list of "English and American Magazines and Reviews," pp. 8–20, we fail to find either the *Annals and Magazine of Natural History*, the *Geological Magazine*, or the *Zoologist*, while in the "Classified Tables" we only find two of them. In the "Index to the Periodicals," pp. 72–180, also, these three periodicals are conspicuous by their absence. As they contain some of the best work in the natural history sciences, and are, moreover, among the recognised channels for British workers, this seems somewhat strange. They should appear in all the lists, and should decidedly be indexed.

In future editions the journal *Yn Lioar Manninagh* should appear under L, not Y.

"MINERVA, Jahrbuch der Gelehrten Welt," edited by Dr. R. Kukula and K. Trübner (Trübner, Strasburg, price 7 marks), has made its appearance for 1893-4. It is a most valuable book of reference for those who wish to know the names and addresses of their fellow-workers, although it does not profess to include those unconnected with some public institution. The present volume is as neat in its get-up as its predecessors, and is embellished with an admirable engraved portrait of Louis Pasteur.

"An Astronomical Glossary or Dictionary of Terms used in Astronomy," by J. E. Gore, has been issued by Messrs. Crosby Lockwood & Sons. Students of Natural Science, especially those interested in Cosmogony, and in theories of climatic change, may be glad of a handy book of Astronomical terms. The definitions are for the most part brief. In the case of meteoric stones—"stones which occasionally fall from the sky"—a little more might have been added. Moreover, we may add the three kinds, Siderites, Siderolites, and Aerolites, were named by Maskelyne, not Denning.

The new edition of the Tunicata in Bronn's "Klassen und Ordnungen des Thier-Reichs" is to form the supplement to bd. iii. (Mollusca), and is edited by Dr. O. Seeliger, of Berlin. Lieferung 1 (48 pp.) lies before us, and is devoted to the first four, and part of the fifth, sections of the historical introduction, which is treated in the same thorough manner that characterises the main section of the volume.

M. Paul Pelseneer's new work, entitled "Introduction à l'Etudes des Mollusques," will, we understand, shortly appear. It is to form a single 8vo volume with 146 figures in the text, and will be published by M. Lamertin, of Brussels, at a price (6 fr.) which should put it within reach of all students of Malacology.

The Royal Swedish Academy of Science has just issued part i. of a detailed monograph of the Silurian Crinoidea of Gotland. This work, on which Mr. F. A. Bather has been engaged for the past four years, should prove of service to the numerous collectors of our own Wenlock fossils.

We have just received a number of publications from the United States Department of Agriculture. That of the most recent interest is volume ii. of illustrations of North American Grasses, dealing with the grasses of the Pacific slope, that is, of California, Oregon, Washington, and the north-western coast, including Alaska. It was prepared by the late Dr. George Vasey, and was, probably, the last official work on which he was engaged, as his letter of transmittal bears a date but little prior to that of his death. It contains plates with descriptions of one hundred of the more important species. The former are among the nicest specimens of grass illustrations that we have seen.

We are glad to learn that Dr. William Fraser Hume's pamphlet on the Cretaceous sediments, which was reviewed on p. 228 of our last volume, has now been published, and can be purchased from F. H. Butler, 158 Brompton Road, London, S.W.

OBITUARY.

PAUL FISCHER.

BORN JULY 7, 1835. DIED NOVEMBER 29, 1893.

BY the death of Dr. Paul Fischer, malacologists have suffered a heavy loss, and one that will not be easily replaced. He was for many years assistant naturalist to the chair of palæontology in the Jardin des Plantes, Paris, also a prominent member of the Commission of Submarine Dredging, and a member of several scientific societies. He does not, however, appear to have been a very ambitious man; and was quite unknown to many of his correspondents except through his writings.

As an author he was most prolific, and although he occasionally wrote on some of the lower forms of vertebrates, the mollusca claimed practically all his attention. And he was not a mere describer of shells; it is quite clear that whenever the animal of an interesting genus fell into his hands, a contemporary journal invariably contained something original from his pen concerning its anatomy. His *Observations anatomiques sur les mollusques peu connus*, a series of articles written in the *Journal de Conchyliologie* in 1856-57; *Ueber die Respiration bei den mit Lungen versehenen Land-Gasteropoden; Anatomie du genre Septifer; Anatomie de deux Mollusques pulmonés terrestres appartenant aux genres Xanthonyx et Hyalimax; Note sur l'anatomie des Cyrènes Américaines*, and many other memoirs of a like nature all testify to his thorough acquaintance with the soft parts of his favourite group. Occasionally we find him writing about bryozoa, hydrozoa, sponges, brachiopods, etc., but his interest in them seems to have been merely a passing one, kindled, perhaps, by the contents of his dredge. Sometimes geology claimed his attention, though this was generally due to some collateral influence. Thus he gives us a *Note sur la géologie du Sud de Madagascar*, in 1868; *Sur l'existence du terrain tertiaire inférieur à Madagascar*, etc. Palæontology does not seem to have had much attraction for him in his minor writings, but his knowledge of the fossil mollusca was comprehensive enough, as we shall presently see.

Altogether he has written more than 300 separate memoirs and papers, and, in addition, 100 memoirs in conjunction with MM. H. Crosse (his co-editor), Delesse, Tournouër, Bernardi, and others, published for the most part in the *Journal de Conchyliologie*.

Among his more important works we may mention the descrip-

tion, in conjunction with M. Crosse, of the mollusca collected by the Scientific Mission to Mexico and Central America; with M. Tournouër, the invertebrate portion of the fossils of Mont Léberon, in Vaucluse; and accounts of the mollusca collected during the voyages of "Le Travailleur" and the "Talisman."

Dr. Paul Fischer, in spite of the numerous writings above-mentioned, is best known as the author of the celebrated *Manuel de Conchyliologie*, published in Paris (1880–87). The appearance of this work at once stamped the author as one of the most eminent malacologists the world has ever seen. It is based on the well-known manual of S. P. Woodward, and in fact the plates illustrating it are identical with those in the English work referred to; but although the plan is the same, it is naturally a much larger work, and must have entailed years of labour. It is here that we first become acquainted with the fact that Dr. Fischer knew the fossil mollusca quite as well as the living forms, and seeing how little he had previously written on the matter, this must have been a surprise to all but his most intimate friends. To say that the work is perfect is to ignore the insuperable difficulties of the subject; but it is now, and will for some years remain, the principal text-book on recent and fossil mollusca.

Dr. Fischer was a man of somewhat reserved habits, but was always willing to render assistance to those who consulted him, and they were not few. He had been ailing for a long time, and passed to his rest on the 29th November last.

G. F. H.

GEORGE GORDON.

BORN JULY 23, 1801. DIED DECEMBER 12, 1893.

WE regret to record the death of this veteran geologist, though of course, at his advanced age, his life could not long have been spared. He was son of the Rev. William Gordon, and was born in the old manse of Urquhart, near Elgin. Entering Marischal College, Aberdeen, in 1815, he graduated M.A. in 1819. After studying Divinity both at Aberdeen and at Edinburgh, he was licensed by the Presbytery of Elgin in 1825, and was presented to the parish of Birnie, by Francis, Earl of Moray, in 1832. For 57 years he was minister of Birnie, retiring in 1889 to Brae Birnie, a house in Elgin. He was made an LL.D. of Marischal College in 1859.

He had early developed a taste for Natural History and Antiquities, and eventually became one of the most enthusiastic of Scottish geologists. In 1839 he published "Collectanea for a Flora of Moray," and later on contributed to the *Zoologist* a series of papers on the Fauna of Moray.

In the great question of the age of the Elgin Sandstones, Dr. Gordon took the deepest interest, and to the last he maintained that

they all belonged to the Old Red group, and that the Reptiles, generally considered to be Triassic, had appeared in that earlier period. To his energy we owe the grand collection of remains of these Reptiles, some of which were described by the Right Hon. T. H. Huxley, and others, even more remarkable in character, quite recently by Mr. E. T. Newton. One of the new genera has been named *Gordonia*. Dr. Gordon was mainly instrumental, some sixty years ago, in founding the Elgin Museum and Institution.

Until within a fortnight of his death, Dr. Gordon was in vigorous health for a man of his years. He was over six feet in height, and consequently a man of commanding presence; while his enthusiasm in Science was maintained to the last, and those who had the privilege of accompanying him to the Elgin quarries, when he was over ninety years of age, will never forget his wonderful energy, his geniality, and intellectual vivacity.

THE deaths are also announced of L. CHABRY, the well known investigator of experimental teratology; of HENRY RINK, author of geographical and ethnological works on Greenland; of D. SCOTT MONCRIEFF, an American ethnologist from Harvard University, who was accidentally drowned while travelling in Siberia; and of JUAN VILANOVA Y PIERA, the well-known geologist of Madrid.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

DR. A. BALDACCI, of Bologna, has returned from a botanical tour in Crete.

DR. A. TERRACCIANO has resigned his post as Conservator at the Royal Botanic Institute at Rome; Dr. O. Kruen has been appointed to fill the vacancy.

THE prize of 1,000 fl. instituted in honour of Dr. T. Mayer has been awarded by the Academy of Sciences in Cracow to Dr. M. Raciborski for his work on the fossil flora of Polland.

The following awards have been made among French botanists: M. Gaston Bonnier to be "Chevalier de la Legion d'honneur," M. Leclere Savlon to be "Officier de l'Instruction publique," and M. L. Planchon to be "Officier d'Academie."

PROFESSOR E. D. COPE has been appointed to the Professorship of Comparative Anatomy and Zoology in the University of Pennsylvania, and Dr. A. P. Brown succeeds him in that of Geology and Mineralogy.

MR. E. A. MINCHIN, B.A., Radcliffe Fellow of the University of Oxford, has been elected to a Fellowship in Biology at Merton College. Mr. Minchin's original work has hitherto lain chiefly among sponges and echinoderms; he has been engaged of late on an English translation of Professor O. Bütschli's monograph on the foam-structure of protoplasm, which has just been issued by the Clarendon Press. We understand that Mr. Minchin will reside in Oxford, continuing his researches, and giving lectures.

MR. ARTHUR WILLEY, B.Sc., now lecturing in Columbia College, New York, has been elected by the special board for biology and geology of Cambridge University to the Balfour Studentship. The first award of the Walsingham medal, founded by the Lord High Steward for the encouragement of biological research, has been made to Mr. E. W. MacBride, who is well known by his work on Echinoderm embryology. Mr. Willey is being sent to New Ireland to investigate the early development of *Nautilus pompilius*. Could not Mr. MacBride be directed to start for Jamaica or Japan to study the embryonic and larval stages of *Pentacrinus*?

WE have received No. 2 of the *Kansas University Quarterly*, which contains two entomological and two palæontological papers, and an account of some experiments on the delicacy of the sense of taste among Indians. The most important of these contributions are Dr. Kellogg's description of the head of a butterfly (*Danais archippus*), and Professor Williston's brief illustrated notes on the skeleton of Pterodactyls and a Mosasaur from the Chalk of Kansas.

THE Duke of Westminster has subscribed the sum of £1,000 to the extension fund of the Grosvenor Museum, Chester. The new portion of the building is already well advanced.

THE Manchester Museum has issued a second edition of its illustrated General Guide. Among the most recent acquisitions is a collection of the Coal-measure fossils from Lancashire, presented by Mr. Robert Cairns. A similar collection was presented some months ago by Sir U. Kay Shuttleworth.

THE Société Industrielle de Mulhouse has just issued a guide to the Mulhouse Museum. Among many other important collections therein contained, we may note the geological specimens of Kœchlin-Schlumberger; the plants and marine fossils of the Culm of Thann, collected by Albert Scheurer; the Debay collection of fossil plants from the Senonian of Aix-la-Chapelle; the quaternary mammals from the Mulhouse district; and the Mühlenbach, Rabenhorst, Schlumberger, and Han herbaria.

WE regret to learn from the recently-issued Annual Report of the Brighton Museum, of the retirement of Mr. Edward Crane from the chairmanship of the Committee. Mr. Crane has served on the Committee for twenty-one years, and was elected chairman eight years ago on the death of Dr. Thomas Davidson. During this long period he has not only rendered inestimable service to the Museum, but has also been one of the foremost promoters of the interests of Science in the town; and his resignation is only due to the feebleness caused by advancing years. There is appended to the Report a scheme for the continuation of the Booth Collection of British Birds. 220 species are already represented, and a special effort is to be made to gradually complete the series.

THE address of the President, Mr. J. F. Whiteaves, to the Royal Society of Canada, section iv., has just been issued separately. It is an account of our present knowledge of the Cretaceous Rocks of Canada.

THE Palæontographical Society announce the preparation of a monograph on the "Anthracosias and Anthracomyas of the Coal-measures," by Dr. Wheelton Hind. As Dr. Hind has already dealt with many of these forms in vol. xlix. of the *Quarterly Journal of the Geological Society*, we have good hopes that this monograph may actually, as the Council promise, "appear at an early date."

THE North Staffordshire Naturalists' Field Club has issued a concise and valuable report on the progress of geological and palæontological research in the district, with a bibliography from 1679 to the present day. It has been prepared by Mr. John Ward, of Longton.

THE Liverpool Geological Association have issued no. 5 of vol. xiii. of their *Journal*. It contains a paper by Mr. J. Herbert Jones on the influence of local geology on the commercial importance of Liverpool, and a short discussion of the metamorphic origin of granite by Mr. R. W. Boothman Roberts.

THE formation of Field Naturalists' Clubs in France seems to be on the increase. We learn from *La Feuille des Jeunes Naturalistes* (December 1, 1893) that a new Natural History Society has recently been founded at Mâcon, while similar societies are in process of formation at Bourg and in Haute-Saône. There is also

an Entomological Association at Armentières, and an Union des Naturalistes de la Seine, which latter has its headquarters at Clichy, and for its principal object the study of entomology.

THE Edinburgh Geological Society have just completed the sixth volume of their *Transactions*. The fifth and concluding part contains several papers of interest and importance, notably an anniversary address by Mr. H. M. Cadell, dealing with the geological changes wrought by man within the Forth basin, in which changes Mr. Cadell and his forbears seem to have played a prominent part. Mr. Cadell also publishes a map of the site of Edinburgh in ancient times, showing no less than seven lakes. This paper, as well as some Notes on a Shell Mound at Tongue Ferry, Sutherland, by Messrs. Peach and Horne, will interest archæologists as well as geologists. The latter authors also draw attention to some Shelly Boulder Clay in North Ronaldshay, Orkney, which bears on the development of the Scandinavian and Scottish ice-sheets during the climax of glacial cold. The part concludes with the first half of an important paper by Dr. H. J. J. Lavis on the Ejected Blocks of Monte Somma, which deals chiefly with those consisting of more or less altered stratified limestone.

THE annual Soirée of the Croydon Microscopical Club was held on the 22nd November, at the Public Hall, Croydon. There was a good attendance, and the exhibits were up to the average of former years. The most noteworthy thing for criticism, however, is the lamentable falling-off in the exhibition of microscopes. Formerly the Croydon soirée was one of the best of the season, and not only did the microscopes fill all the tables in the large hall, but the exhibition of objects was first-rate. For the last few years the microscopes have been dwindling in number, and the exhibition has become poorer and poorer, until we now see one complete table in the hall filled with butterflies, and, with but few exceptions, as poor an exhibition of microscopy as can be imagined. The objects are inferior, the manipulation of light is bad, and all the old prestige of the Croydon as a microscopical club seems tottering to its fall. Cannot a few energetic members pull it together once again, give a few lessons in illumination and general technique to the members, and restore the microscopical and the more scientific element?

CORRESPONDENCE.

Emendations of Nomenclature.

With all due deference to the Editor, I must confess to agreeing with "Macaulay's Schoolboy," that in certain cases the emendation of generic and specific names is not only pardonable but imperative. Such small alterations as the addition of an aspirate or the change of a letter can scarcely affect the intelligibility of a name. Take, for instance, *Oxynoticeras* which should be *Oxynotoceras*,—*serrodens, serridens*,—*heterogenum, heterogenes*. These alterations are desirable, though I own that, copying others, I have allowed the incorrect forms to pass. Again, what warrant is there for the declension of *nanus* and *capricornus* as adjectives, giving them the endings *a, um* when they are substantives? Is not *Straparollus* derived from a man's name, and, therefore, to be written *Straparollia*, so as to avoid sending anyone on a wild-goose chase through Latin and Greek dictionaries for its derivation? As to more serious alterations, the proposed changing of barbarous compounds of a man's name with *ichthys* or *ceras* into mere *ia*, I personally agree with, but I consider, if they are to be carried out, that there should be a statute of limitations in those cases—say 20 years; and that older names should be exempt. Although scientific nomenclature is for scientific men, it is certainly desirable that they should do their best to conform to classical usage.

S. S. Buckman.

Elementary Zoology.

Can you or any of your readers recommend me a cheap but correct book dealing with the general anatomy of common marine animals, such as are commonly preserved in the fossil state? I would specially instance the mussel, cockle, oyster, whelk, cuttle-fish, sea-urchin, sea-anemone (as an example of an Actinozoan), crab, lobster, some brachiopod and *Flustra*, or any common Bryozoan. One is constantly being asked about some such book, but the few that I know are either too expensive, too old-fashioned, or too technical for the beginner.

207 Harrow Road, W. F. A. Bather.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

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